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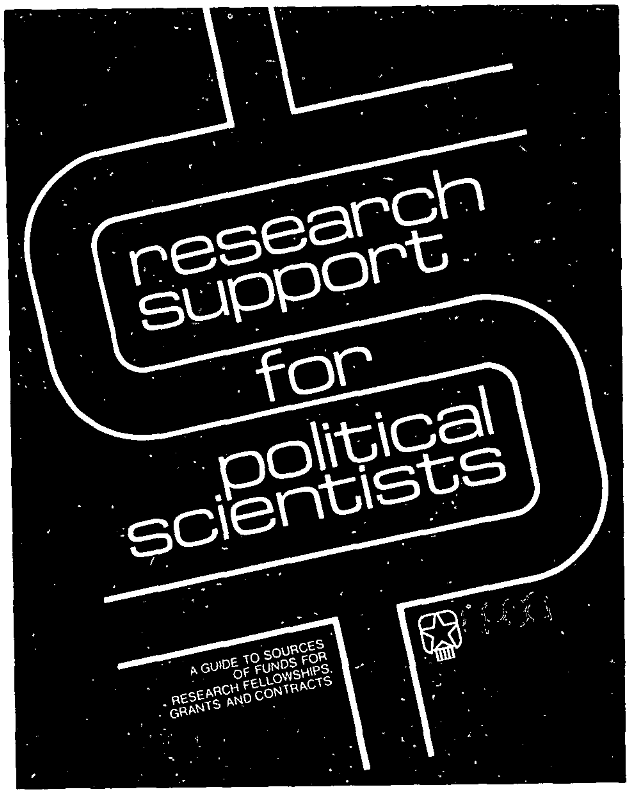
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Federalism, Nationalism, and Democracy in America*

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Presidential Address, American Political Science Association, 1977

The growth of the public sector in recent years has been accompanied by both centralization and decentralization. More important than any such shifts of power or function between levels of government, however, has been the emergence of new arenas of mutual influence among levels of government. In this way recent developments in intergovernmental relations are adding a new dimension to the national system of representation. That the federal division of powers should serve a representative function is entirely in accord with the original design. A look at this design in the light of the theory of its founders helps one perceive this distinctive purpose. Then as now American federalism was primarily an instrument of national democracy. Current American experience with this aspect of intergovernmental relations, moreover, suggests questions for comparative study of the contemporary welfare state.

During the 1960s in the United States, as in most advanced countries, there was a large and sudden surge upward in the growth of the public sector, largely under the impetus of the central government. Supporters of this new phase of centralization sometimes see it as one more, and perhaps the final, stage in the transformation of the American polity into virtually a unitary system (Sundquist 1969:6–13; Reagan 1972:163). Critics, on the other hand, seek to revive federalism, frequently proposing a reallocation of functions between levels of government (Nathan 1975:111–15; Elazar 1973:284, 292). Some find evidence of such a revival in the decentralization accomplished by recent less restrictive schemes of federal aid (Nathan 1977:131, 164–65).

I should like to present a third position. My thesis is that more important than any shifts of power or function between levels of government has been the emergence of new arenas of mutual influence among levels of government. Within the field of intergovernmental relations a new and powerful system of representation has arisen, as the federal government has made a vast new use of state and local governments and these governments in turn have asserted a new direct influence on the federal government. What is interesting about American federalism today is not its particular allocation of functions or powers between levels of government, but rather what it is adding to our national system of representation.

Moreover, I should say that these developments, while new, are in harmony with the original design of the federal system. Federalism as the mutually exclusive allocation of powers between the general and the state governments—dual federalism, it may be called[1]—belongs to the past. Dual federalism was indeed a feature of the original design and, broadly speaking, characterized our system of multilevel government until the New Deal. Federalism in this sense does not apply, and given the realities of our times, could not be made to apply, to that system today.

But dual federalism was only one and a secondary feature of the original design. The theory governing that design made its territorial allocation of powers an instrument of a far more important purpose. That purpose was so to divide and organize power as to avert the evils and realize the benefits of free government. Within this general scheme, the federal division of powers served a representative function by creating a structure of mutual balance and influence between the two main levels of government. In this manner, dual federalism was a means to representational federalism (Eulau, 1973 and see footnote 5 below).

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*I wish to dedicate this paper to the memory of Martin Diamond from whom over the years I have learned much about American federalism and especially about the importance of theory to its original design.

[1] A mutually exclusive allocation of powers can be conceived as originating in at least two ways. The constituent power may endow the federal government with powers A and the state governments with powers not-A. Or it can endow the federal government with powers A and the state governments with powers B. Corwin coined the term "dual federalism" to refer to the second conception which for a hundred years after Marshall the Supreme Court frequently followed by holding that the Tenth Amendment was a separate grant of powers and therefore an independent limit on Congress. (Corwin, 1934:47–48; 1950:5, 15; 1974:369–72). The term has also come to be used more broadly to refer to the first conception, which would include the Marshallian view that the Constitution granted certain powers to the federal government, leaving the residuum, whatever it may be, to the states (Elazar, 1962:13–21). In Marshall's day and for long after, that residuum was considerable (Grodzins, 1966:24–30). In this sense, one can say that dual federalism characterized our system until the New Deal.

In the first part of this article I shall, therefore, be talking about the original federal design, in order, in the second part, to direct attention to the way that representational federalism has been reconstituted by recent developments in intergovernmental relations. Since these developments have parallels in other countries, I shall conclude with a few questions for comparative study suggested by American experience.

The Original Federal Design

The Democratic Purpose. To see the original federal design as the solution to a problem of representation links it directly and intimately to the great cause that led to the American Revolution.[2] That cause was liberty. To the colonial dissidents liberty meant certain personal rights, such as freedom of conscience, but above all political liberty, the right to government by "the consent of the governed." And by "consent" they meant not some presumed agreement to a form of government delivered by social contract in the distant past, but rather a consent that closely and actively joined voter and representative (Bailyn, 1967:172–75). Ambiguities in this idea permitted various degrees of control by voters over representatives, ranging from strict delegation to a fiduciary relationship subject to frequent accountability. Sometimes the two tendencies were mixed, as when the town meeting of Boston in May 1764 at the height of the Stamp Act crisis delegated to their representatives in the provincial assembly "the power of acting in their public concerns in general as your own prudence shall direct you, always reserving to themselves the constitutional right of expressing their mind and giving you such instruction upon particular matters as they at any time shall judge proper" (Pamphlets:471). One part of the charge leads toward legislative discretion, the other toward popular sovereignty. Either makes government crucially dependent on the will of the voters.

Whether in a more or a less radical form, government by consent as understood by the colonial dissidents was fundamentally and irreconcilably in conflict with the theory and practice of British government in the eighteenth century.[3] The Old Whig constitution had emerged from a medieval past and, although the philosophical and sociological foundations of the polity had been transformed by modernity, John Adams was quite right to see in it powerful remnants of the canon and feudal laws. This polity was hierarchical and corporatist and its hierarchy and corporatism did not depend, and were not believed to depend, upon the will of the voters. From the premises of the system virtual representation and parliamentary sovereignty followed logically. The polity included a great variety of corporate groupings, functional and territorial, often of unequal value and authority. Representation in the House of Commons was adjusted to this ordering. For the individual to be represented, it was not necessary that he actually have a vote for a member of Parliament, but only that persons belonging to his rank or order and residing in his sort of community should have a vote. Indeed, according to these beliefs, if all the people had been enfranchised and grouped in constituencies that would give their votes equal weight, essential differentiations of value and authority would be destroyed. For the same reason, the sovereign authority could not be located in the people, but only in that complex body, the Parliament, which, including King, Lords and Commons, brought all elements of the polity into a common deliberation in their proper ordering. The American dissidents, seized with a different ideology, which descended from that failed democratic revolution of the English seventeenth century, the Commonwealth (Lindsay, 1943: Ch. 5; DeGrazia, 1951:21–28, 53–61), were impelled into fundamental conflict with this Old Whig regime and its rulers. The polemic in the decade or so before the Revolution rightly focused this conflict between hierarchy and democracy on the question of representation.

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[2] In this historical section I have been much influenced by the democratic nationalism of George Bancroft. While an undergraduate I was explicitly warned off Bancroft and introduced instead to the views of Van Tyne, an elegant incrementalist with a strong bias toward the compact theory (1907; 1922:Ch. 1; 1929:Ch. 15). It was therefore something of a revelation to discover some years later the powerful and illuminating perspectives of the earlier writer. The composition of his history stretched over more than half a century, the first volume being published in 1834 and the final revision of the whole work in 1888. For his main themes, see, for example, in that edition, Vol. 1, Ch. 11, "Prelates and Puritans" and Ch. 19, "The Place of Puritanism in History"; Vol. 3, Ch. 9, "The Day-Star of the American Union"; and Vol. 6, Book 3, Ch. 3, "The Connecticut Compromise."

Bancroft's emphasis on the role of ideas in the Revolution, although not necessarily his derivation of these ideas from the Reformation, is supported by Louis Hartz' masterly study of the influence of Lockean Liberalism on American beginnings (Hartz, 1955:Chs. 1 and 2). In general I follow Bernard Bailyn's views of the source of the ideology of the Revolution and of the importance of this ideology in defining the issues of the conflict (Bailyn, 1967: esp. Ch. 5). On the origins of American federalism, however, I depart from Bailyn, who follows McLaughlin (Bailyn, 1967:209n) with whom I strongly disagree (See n. 4 below).

[3] I have discussed the theory and practice of British politics in the eighteenth century, especially in relation to representation, in *British Politics in the Collectivist Age* (1969). The contrast that I draw between what I call Old Whig politics and Liberal and Radical politics in chapters 1 and 2 also marks out the main lines of ideological conflict between the American dissidents and the apologists in America and Britain for the policy and rhetoric of British Administrations from the Sugar Act to the Declaration of Independence.

To give such emphasis to the democratic character of the American Revolution is to play down its national character. One could argue the opposite case, saying, for instance, as Schumpeter did (1947:216–17), that by the middle of the eighteenth century most Americans had come to regard the British rulers as foreigners who were interfering with American interests, but that, perhaps because the ideology of nationalism had not yet appeared, the Americans sought to legitimate with democratic principles what was really a national uprising. This hypothesis may gain some plausibility because of the primacy of nationalism over democracy in the creation of many new states in recent decades. It does not, however, fit the facts of the American case. Given the depth of the conflict one might well conclude that an ultimate decision for independence was inevitable. Some of the colonial leaders came to that conclusion rather sooner than others. But there is no reason to doubt the sincerity of the dissidents as a whole when through a decade of bitter debate they steadfastly claimed that they sought only freedom within the British empire, not freedom from it. For months after Washington took command of the Continental Army in 1775 he and his officers daily drank the king's health.

The federal question did not arise until independence had been chosen. Naturally. One could not seriously consider how or why to allocate powers between a central government and various provincial governments in an American polity until it had been decided to have an American polity. The prerevolutionary debate, therefore, although much concerned with the specific powers that Parliament might exercise in comparison with those of the colonial assemblies, showed no preference for a federal arrangement.[4] Indeed, the first major proposal put forward by a colonial spokesman embodied a unitary, not a federal solution to the problem of imperial government, when James Otis in 1764, echoing an idea favored by Franklin ten years earlier, proposed "an American representation in Parliament" (Pamphlets:445). In his scheme, which was said to have been "universally approved" of in the colonies (409), although insisting that the colonial legislatures be maintained (445–46), he conceded parliamentary sovereignty and granted that the provincial legislatures were unquestionably subordinate to that of Great Britain (449, 452).

In the following years, various plans were offered for a division of powers between the imperial and the colonial legislatures. These efforts to "draw a line" were based on such rationales as the distinction between internal and external taxation (Stephen Hopkins, 1765, in Pamphlets, 512; Benjamin Franklin, 1766:421, 424, 445–46) or between the power to regulate the trade of the colonies and the power to tax them (John Dickinson, 1767:146–47, 156–57). If one may call these schemes federal, one must, on the other hand, recognize that on the eve of independence they succumbed to a proposal which was once again unitary insofar as it denied any and all authority to Parliament over the colonies. This was the idea—of what much later came to be known as "dominion status" (Smith, 1956:58)—that the colonies and Britain were "distinct states" within the empire connected with one another only by having the same king (James Wilson, 1774:745n; John Adams, 1774:37–38, 105, 170). This view of the juridical status of Parliament was carried over into the Declaration of Independence which, turning its full attention to justifying a break with the king, dismissed Parliament as never having exercised legitimate authority over the colonies. No one of the many and various schemes for imperial reorganization had solved the conflict for the simple reason that the conflict was not about the territorial allocation of powers, but about the democratic basis of power.

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[4] I can clarify the thrust of this and the next paragraph by noting how radically my interpretation differs from the familiar and widely held views of Andrew C. McLaughlin who in this *Review* in 1918 argued "first, that the essential qualities of American federal organization were largely the product of the practices of the old British empire as it existed before 1764; second, that the discussions of the generation from the French and Indian war to the adoption of the federal Constitution, and, more particularly, the discussion in the 10 or 12 years before independence, were over the problem of imperial organization" (McLaughlin, 1918:215). Nor, a fortiori, can I accept the theses of his constitutional history that "the two main achievements" of the Revolution were "the establishment of limited government and the founding of the federal state"–his conception of federalism being dual federalism (McLaughlin, 1936:5). I find rather that the arguments of the dissidents emphasize not limited government, but self-government, and that accordingly the meaning they gave to federalism was primarily representational, not dual.

In contrast with McLaughlin's emphasis on law and historical experience, this interpretation stresses theory and ideology. At the same time I would strongly insist that historical experience and the embodiments it had given to English liberty were crucially important in that they lent concrete meaning to the abstractions of the debate. On neither side were the contestants mere ideologues. The conflict of their ideals was sharper for having a specific, historically informed content.

The National Question. When this failure had led to independence, the national question came suddenly and urgently to the fore. What was the source of the authority of the new government? Obviously, the consent of the people—that and nothing more. But was this consent given by one people or by several peoples, by the nation or by the states? The Declaration of Independence described itself in Jefferson's words as the act of "one people" and eleven years later the Constitution similarly declared that it was ordained and established by "We, the people." According to Abraham Lincoln, the unity of the nation asserted in these documents dated back to the time of the Continental Congress when the colonists chose to form the Union, which is, in his words, "older than any of the States, and, in fact . . . created the States" and "produced their independence and liberty" (1861:394).

This version of events provides the historical basis for the national theory of American federalism. According to that theory, a single sovereign power, the people of the United States, created both the federal and state governments, delegating to each a certain limited authority. In this theory of the juridical basis of the American polity popular sovereignty appears as a single national will acting as the constituent power. The compact theory, on the other hand, takes a different view of the same events and arrives at a different view of the Constitution and of federalism. According to this view, the colonies became separate, independent polities when they cast loose from Britain and only thereafter entered into an agreement to have a general government for certain limited purposes. From this theory justifications have been deduced for secession, interposition, or at least extensive "states' rights."

The national theory, I should say, is a superior interpretation of what actually happened, an interpretation incidentally which has been given further powerful support by recent historical research (Morris, 1974:1068–88; Rakove, 1975:Chs. 2, 3 and 4). The important thing, however, for the present inquiry into the original design of American federalism, is that the men who conceived and elaborated that design worked from the premises of the national theory (Morris:1064, 1073). Their federalism presupposes their nationalism. In their view the constituent power was one people, the nation. What they sought to produce in the constitution of the new polity was a scheme by which that nation would act not only as the constituent power, but also as the continuous controlling and directing influence in the political life of the new polity. In seeking to give such effect to this idea of national democracy, they were consistently carrying out the belief in government by consent that had rationalized and impelled the resistance to Great Britain in prerevolutionary days. The problem of representation which had preoccupied the energies of that long struggle continued to be central to the shaping of the federal structure.

The Framers' Theory. Theory had powerfully directed their labors of resistance and theory powerfully directed their labors of construction. In the later as in the earlier phase, the orienting ideology consisted of those same liberal democratic ideas that had come down from the seventeenth century. In their work of construction, however, the Americans also made wide use of "the new science of politics" which had blossomed in the early eighteenth century (Ranney, 1976:141–42; Adair, 1957:347–48). This new study was by no means conceived as value-free, but was dominated by the new hope of the time, free government. Its main concern was how to protect liberty by dividing and balancing power within a polity. The premise of this concern and of its consequent technique was a certain distrust of human nature. Although many of these authors, as in the case of the Americans, had had a Protestant upbringing, I would not say that this distrust was Calvinist—it did not approach that black despair—nor even that it was distinctively Christian or biblical. It was more modern, more secular, a workable pessimism which saw a love of power in all men and feared any monopoly of the instruments of power, but which held that if control over these instruments were properly divided and balanced, power could be made to check power so that it would be used only for the common good. One technique was a separation of powers according to the "natural" functions of government or according to the ranks and orders of society. Another consisted in a division of powers between a general government and a number of provincial governments. It was, of course, this latter sort of balancing that the framers sought to achieve in their federal design.

That design was unique. At Philadelphia in 1787, it is generally recognized, the Americans invented federalism as it has come to be understood since that time (Diamond, 1961:21–23). The scheme had no precedent (Federalist 37:226). Its authors were not, as some have claimed (McLaughlin, 1918:238–40), attempting to restore an allocation of powers between central and provincial governments like that the colonists had experienced in the days of "benign and salutary neglect" under the old empire before the Sugar Act. They were surely not trying to imitate the Dutch, Swiss or German regimes, a type of polity which they regarded as "the cause of incurable disorder and imbecility" (Federalist 9:52; Riley, 1968:401). Their new creation was theory-based. Yet even in drawing generously on the political science of their time, they did not follow their authorities slavishly. Indeed, their inventiveness consisted precisely in combining elements taken from two incompatible constructions of, respectively, Montesquieu and Hume.

Montesquieu's contribution came principally from his famous discussion of the confederate republic in Books VIII and IX of *The Spirit of the Laws.* There he was trying to reconcile the conflicting conditions conducive respectively to liberty and to security. In conformity with the conventional view, he held that "republican government" could flourish only in a small state where "the public good" is "more obvious, better understood, and more within the reach of every citizen." In an extensive republic, on the other hand, although its defensive posture would be stronger, the public good would be "sacrificed to a thousand private views" and encroachments on liberty would be able to grow without arousing general resistance (120). Distance, size or—more properly—scale would present the man of ambition with his opportunity and his temptation. Montesquieu proposed to realize the respective advantages of smallness and largeness of scale without introducing their disadvantages by means of a confederate republic. Formed by "a convention" among a number of small republics, such a polity could amass defensive power without making itself vulnerable to the internal corruption of despotism, because the member states would retain the independent force to prevent the abuse of power by the general government. The territorial pluralism of these continuing small governments would counteract tendencies toward corruption in the wider polity (126–27).

Montesquieu's confederate republic alone was clearly not the model for the constitution makers at Philadelphia. It was rather the sort of regime which in their eyes had proved to be both inefficient and dangerous to liberty under the Articles of Confederation. Their radical transformation was to impose on it certain features of a unitary regime. For the sake of "stability and energy" (Federalist 37:227) they gave the general government a new instrument of power by enabling it to act directly on individuals. But their main concern (Madison, 1787:273–79; Convention: 75–77; 88–89) was to add a further protection of "republican liberty" by providing for the representation of individuals in the federal legislature.

The significance of this new scheme of representation was given its classical exposition in Madison's Tenth Federalist. His idea is an adaptation of a proposal put forward by Hume in his essay on the "Idea of a Perfect Commonwealth" (Adair, 1957:348–49, 353). In that essay, first published in 1752, Hume sketched an elaborate system of representative government and against this background attacked the "common opinion" that republican government is more likely to survive in a small than a large polity. On the contrary, he argued, the "near habitation" of the citizens of a small polity will make even their division into small parties vulnerable to "the force of popular tides and currents." In the large representative republic, however, not only will the "higher magistrates" "refine" the opinions of the voters, but also the various parts will be less likely to unite against "the public interest" (Hume: 513–15). Madison improves on this model by stressing the diversity of social and economic interests that will be embraced in the more extensive republic. Thanks to the greater differentiation that goes with larger scale, the social pluralism of the general government will counteract tendencies toward a factional abuse of power in the subordinate governments.

The invention at Philadelphia transformed Montesquieu's model by integrating with it this Humean construction. The new unitary features meant that now the social pluralism of the nation as a whole would be represented in the general government, which, within limits, would be able to avert the dangers of faction within the states, while the continued existence of the states meant that, as in Montesquieu's model, territorial pluralism would constitute a safeguard against encroachments by the general government. It was a unified, internally coherent and highly original model of a new kind of government. This invention resulted from compromise, to be sure—not the compromise of stalemate, however, but of social learning.

Federalism as Representational. In the *Federalist* papers and the ratification debates, the new model was set forth and defended. It had a military version in which the possession of instruments of coercive force by each level held the balance for free government (Diamond 1961:55; Federalist 46:309–11, 60:390). "Power being almost always the rival of power," wrote Hamilton, "the general government will at all times stand ready to check the usurpations of the state governments, and these will have the same disposition towards the general government." His next sentence revealed the motor in the mechanism: "The people, by throwing themselves into either scale, will infallibly make it preponderate. If their rights are invaded by either, they can make use of the other as the instrument of redress" (Federalist 28:174). In short, the same force which, according to the national theory, had brought the Constitution into existence and formed its juridical foundation, the sovereign people, would continue to guarantee its free operation.

This military version of how the federal design would operate is hardly more than an historical curiosity today. But its authors were, of course, also thinking in larger terms and of a more political application. They expected the social pluralism of the general government to operate not only in emergencies, but also in day-to-day decision making. Nor did they see this function as merely negative—to prevent narrow and oppressive majorities from forming or acting. Their political science taught them that "the larger the society . . . the more duly capable it will be of self-government" (Federalist 51:341). They therefore expected that the majority coalitions which did form within the general government not only would respect "the rights of every class of citizens," but also would positively express "principles . . . of justice and the general good" (340–41). The framers were not some sort of early-day laissez-fairists. Indeed, Turgot gave them a famous scolding for their interventionism. They lived in an age of state-building and mercantilism and fully recognized the need for active government in their developing economy. They saw no need, however, to trade off liberty for development and, although by no means utopians, they had high hopes for their political engineering, believing that its processes of mutual balance and influence would not only break the violence of faction, but also produce decisions worthy of general assent.

In the *Federalist* papers and the ratification debates, discussion of the military aspects of the federal balance shade off into a more political version, which tells us a good deal about how the American system has actually worked and which is still vividly relevant to its operation today. "Notwithstanding the different modes in which [the federal and state governments] are appointed," Madison wrote in the 46th *Federalist*, "we must consider both of them as substantially dependent on the great body of the citizens of the United States. . . . The federal and the State governments are in fact but different agents and trustees of the people, constituted with different powers, and designed for different purposes." "The people" is "the ultimate authority," the "common superior" of both. Nor does he mean this only in the sense of juridical foundation or military balance. He is also concerned to show how the people, acting as the common electorate of all levels of government, bring state perspectives to bear on federal decisions and federal perspectives to bear on state decisions. The three main propositions in his analysis are that these perspectives will not merely reflect the immediate wishes of the voters, but will be shaped by the processes of self-government in which the voters take part (Federalist 56:366–68; 10:59; Huntington, 1959:200; Eulau, 1974:161); that the influence between levels will pass from state to general government, but also from general government to state government (Federalist 46:306–08, 53:351, 56:368; Madison, 1787:275; Convention:74; Diamond, 1961:48–50); and finally that the medium through which this influence will be transmitted will be the common electorate of the two sets of governments (Federalist 46:306; 308–09; 57:371; Eulau, 1974:162).

The essence of the invention of 1787 was the use of the same electorate to choose two sets of governments, each with constitutional protection. As in the military version of the new federal system, where the people were to maintain the balance for free government by casting their weight in one or the other scale, in this political version the medium of interaction was the common electorate. Governing himself through two different governments, the voter views the political world from two perspectives, one shaped by the social pluralism of the general government, the other shaped by the territorial pluralism of state government. In his political life, as a member of one nation, he does not separate from one another the two perspectives and the interests each elicits in him. His state perspective affects his choices and decisions in federal politics as his federal perspective affects his choices and decisions in state politics. One may call this process "representational federalism" because it gives representation in the general government to the territorial pluralism of the states and representation in the state governments to the social pluralism of the general government.[5]

In framing and debating the new federal structure, Americans of the time were concerned with the same central problem that had stirred them to criticism, resistance and rebellion a generation before: the problem of representation. Now as then their interest in the allocation of specific powers between levels of government was secondary to this overriding concern. They did believe that certain government functions were more effectively exercised at one rather than the other level. Defense, for instance, was more properly a function of the general government. Yet, guided by their primary concern for liberty, they did not hesitate to divide authority over this function, giving important military powers to the states.

Federalism has often been advocated primarily as a means of accommodating levels of government to territorial diversity. The compact theory would lead one to expect this to be the major subject of debate during the framing and ratification of the Constitution. Supporters as well as critics did recognize that the "sentiments, habits, and customs" of the states were diverse and that, therefore, "a government which might be very suitable for one might not be agreeable to the other" (Elliott 2:199). Yet the great mass of utterance at Philadelphia and the ratifying conventions displays remarkably little concern with this fact. Even the most ardent champions of greater powers for the states gave little or no weight to the argument from territorial diversity. Luther Martin, for instance, did not ground his advocacy of state power in his identification with Maryland as a distinctive community or in its need for authority commensurate with its special values or way of life. When he attacked the new powers of the federal government, he, as much as Madison or Hamilton, saw liberty as his goal and the new science of politics as the means for reaching that goal (Elliott 1:348–49, 351). He differed from them only in clinging to the conventional wisdom of the time as put forward in an unalloyed version of Montesquieu's theory of the confederate republic (353). It is consistent with this ground of the differences among Americans of that day over the Constitution that a bill of rights, which protected the liberties most valued by their common ideology and which was, as John Hancock said, "in no wise local, but calculated to give security and ease alike to all the states" (Elliott 2:175), served to win over critics and produce the quick subsidence of opposition that followed ratification.

New Structures of Representational Federalism

In summary, my historical thesis is: that in making a democratic revolution, the American rebels created a nation and invented representational federalism as a means of governing their new national democracy.

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[5] I claim no originality for this view of American federalism. It is simply a restatement of the classical liberal view of the function of the division of powers in popular government. I have avoided, however, the laissez-faire deviation which conceived of this function as being merely the restraint of government action (Friedrich 1968:195–96). My interpretation has many points in common with that of Samuel P. Huntington (1959). He also sees not mere balance, but continual political interaction between the two levels of government (192) and stresses not only Madison's expectation that state governments would have influence on the federal government, but also that "from Minnesota railway rates to Little Rock schools, the national government, in keeping with Madison's principle, has frequently intervened to protect local minorities against local majorities" (198).

I have taken the term "representational federalism" and much of the analysis presented by Heinz Eulau (1974). I have adopted his notion that intergovernmental influence is exercised through the "shared" electorate, a conclusion which need not be based on inference only, as Eulau says (162), but is clearly what Madison meant (Federalist 46:305, 306). I depart from his interpretation mainly in finding that Madison expected not only that the states would influence the general government, but, also that the general government would influence the states.

Representational federalism is not the same as cooperative federalism. The latter term refers to direct relations between levels of government, as in state or local implementation of federal grant in aid programs. The term was coined around 1938 (Graves, 1964:821), when during the New Deal the federal grant system was being vastly expanded and in this and other ways the old pattern of dual federalism was being brought to an end (see above note 1). Representational federalism is compatible both with dual federalism and, as I shall argue in the second part of this paper, with cooperative federalism.

A Rationale for the States. The reason for looking at this history and especially at its theoretical component is that they tell us something important about how the American polity has actually worked and continues to work. This perspective, in the first place, throws light on what we can and cannot expect of the states today. Any modern polity will have one or more levels of government. The smaller governments may be designed simply as administrative districts under the central authority. They may be set up for an economic purpose, and, accordingly, endowed with powers and boundaries suited to a distinctive complex of agricultural, commercial or industrial activity. They may be so laid out as to match patterns of cultural differentiation, as in the case of linguistic boundaries or other indicators of diversity in community values.

None of these rationales, administrative, economic or cultural, makes sense of the American states, except occasionally and accidentally. Look at the map. It must make you wonder whether there could have been a United States, if the rectangle had not been invented. Typically, those boundaries were not laid out to fit some pre-existing community of value or complex of interests, nor has it been possible to adapt them to territorial diversities as these have emerged. Most of the boundaries were dictated by Act of Congress, usually when the area was sparsely populated and had only the status of a territory. If the purpose of the states had been to provide a level of self-government functional to territorial diversity, then it would have been imperative, in this rapidly growing and developing society, that their boundaries be changed from time to time. On the contrary, however, our national policy toward federalism has been to freeze the boundaries of the states into a virtually unchangeable form by giving them constitutional protection.

Michael Reagan observes that the constitutional meaning of federalism still has importance in only one respect: the guarantee of the independent existence of the states (1972:158). This may be so, but it does not mean that federalism is dead. Such a guarantee, to be sure, is dysfunctional to an administrative, economic or cultural role for the states. It is, however, highly functional and, I should say, indispensable to their political role in representational federalism. Even if state and federal power were completely overlapping, even if our society were perfectly homogeneous, it would still be necessary, in the light of the original design, that state government have its constitutionally protected existence. The rigidities of our federal system may often frustrate the purposes of public administration, economic efficiency and community living, but they make political sense as the foundation of a major and distinctive element of our representative system.

The Dual Role of Party. Over the course of time, both state and general governments have performed in various ways the roles assigned them in this system by representational federalism. When one asks what specific forms these processes have taken, the answer, until very recently, I suggest, would be found mainly in the mode of operation of the major political parties. The original federal design endowed the voter with two basic roles, a federal role and a state role. Typically, any major American party has reflected this dualism. The territorial pluralism of the federal structure has had such great and obvious effect as to lead us often to speak of the parties as coalitions of state and local organizations. At the same time, we recognize that their participation in the politics of the general government draws them into a competition which addresses problems and appeals to group interests transcending state and local boundaries. In spite of the resulting territorial and social pluralism, each party is also national, as a body of voters possessing at all levels of government common symbols which focus sentiments of party identification and ideas of party principle. This affectual and cognitive identification is a bond of cohesion that helps make each party a forum within which federal and state perspectives mutually influence one another, instead of merely finding expression in separate spheres.

The politics of civil rights during the past generation provides striking illustration of how the Madisonian mechanism may work through the medium of party. I will merely suggest the outlines of this very complicated process. After World War II, the movement of southern blacks to Northern cities admitted them for the first time to effective political participation in the social pluralism of the more extensive republic. The competition of Republicans and Democrats for this vote (Brauer, 1977) led to intervention by the federal government to remedy the denial by state governments of rights generally enjoyed by American citizens (Wilhoit, 1973:201–12). At the same time, within the Democratic party an interaction between federal and state levels was producing political changes tending toward the same result. Action by the national party organs supported and stimulated within some southern state parties the rise of loyalist groups favorable to civil rights. These groups fought the old leadership, won power in state parties and influence on state government and so were able in some degree to ease the acceptance of federal initiatives. Thanks to the party system the pluralism of the more extensive republic helped bring about a universalistic result.[6]

Public Sector Politics. I have made this brief reference to the way that representational federalism has worked through the medium of the party system in order by contrast to bring out the significance of a recent and major change.[7] The party system operates outside government to bring influence to bear on government. Recently, however, powerful new centers of influence on what government does have arisen within government itself. These new centers of influence arise and act within what one may call the public sector of the polity. For the polity like the economy has a public sector. The private sector of the polity consists of people in their private capacities trying to influence government action. This includes their activity as individuals, as members of organized groups, as adherents of political parties. The character of the public sector of the polity is evident by contrast. It consists of people in their public capacities trying to influence government action: a chief executive vetoing a bill, legislators logrolling appropriations, bureaucratic experts developing new programs, mayors lobbying Congress for more federal aid.

Within this public sector of the polity two types of influence have been on the rise during the past decade or so. One results from functional specialization in the modern state. I call it the professional bureaucratic complex (Beer, 1976:157–66). The other results from territorial specialization and I call it the intergovernmental lobby (Beer, 1976:166–71). The action and interaction of the professional bureaucratic complex and the intergovernmental lobby constitute the new form taken by representational federalism.

The growing role of professionalism in government is so well recognized as to need only a few words to show its relevance to the present discussion. People with scientific and technical training have been important to the modern state from its very beginnings. But during the past generation or so scientific advance has proceeded at such a vastly increased rate as to give "professional specialisms" (Mosher, 1968:105) a new role in policy making (Beer, 1973:74–80). The term "professional bureaucratic complex" is singular: the examples are many. One is the "military industrial complex," from which I adapted the name. A similar structure is shared by the "health syndicate," the "educational establishment," the "highway lobby." The main component in any such complex is a core of officials with scientific or professional training. This bureaucratic core also normally works closely with two other components: certain interested legislators, especially the chairmen of the relevant specialized subcommittees, and the spokesmen for the group that benefits from the program. Such a tripartite complex or subsystem is Seidman's "iron triangle" (1970:37).

The intellectual history of federal domestic programs since the days of the Great Society is deeply marked by the influence of such complexes of professional expertise. I do not mean to exclude the continuing influence of more familiar political agents, such as an activist president responsive to problems and to the suggestion of problems. But I would remark how rarely additions to the public sector have been *initiated* by the demands of voters or the advocacy of pressure groups or the platforms of political parties. On the contrary, in the fields of health, housing, urban renewal, transportation, welfare, education, poverty, and energy, it has been, in very great measure, people in government service, or closely associated with it, acting on the basis of their specialized and technical knowledge, who first perceived the problem, conceived the program, initially urged it on president and Congress, went on to help lobby it through to enactment, and then saw to its administration (Beer, 1976:160–62). Scientific and technical knowledge by its nature is specialized and the prodigious increase in the number of categorical grants-in-aid to state and local governments is an indication of the rising influence of professionalism. In 1962 there were some 160 separate categorical programs (Walker 1975:134). The catalog of federal grants in aid for 1976 lists 1,030. The amount of aid rose in proportion from $7 billion in 1960 to $60 billion in 1976 (CGO, 1975:25).

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[6] One of the more remarkable examples of this process is the emergence of a unified Democratic party in Mississippi. This struggle began shortly after World War II, reached a peak of intensity during the sixties and culminated in an incredible denouement when in 1976 Governor Finch, formerly an arch-segregationist, joined with Aaron Henry, an original member of the Mississippi Freedom Democratic Party, to integrate the state party organization. An explicit element in the suggessful strategy of the loyalists was to use their influence with the national party as leverage on the regulars to gain a measure of power in the state party. There is some analysis of this process in Fortenberry (1972:493–95) and Bass (1976:207). But I am depending mainly on the detailed analysis of a long, unpublished paper by a graduate student of the Harvard Government department (Stekler, 1976).

[7] In the rest of this section of this paper, I am largely summarizing results of research done for an analysis of the politics of the passage of the general revenue sharing act on 1972 (Beer, 1976). Rather than citing again the evidence brought forward there and the references to other work, I shall for the most part refer to the relevant pages of that article and to some other work of mine. I regret the appearance of self-advertisement, but this procedure seems to suit a summary and certainly makes for easier reading.

This expansion of the public sector was accomplished by an unprecedented use of the federal structure. With few exceptions the new specialized programs were not administered directly by the federal government, but by means of state and local governments. One might expect such attempts to join bureaucratic agents at different levels of government in the administration of specialized programs would create awkward inter-level conflicts. Actually, among similarly trained professionals their common discipline has facilitated cooperation, helping them stand off the claims of rival disciplines and the directives of coordinating authorities. Vertical bureaucratic hierarchies cutting across different levels of government have become a main feature of the present phase of American federalism.

Technocracy and Topocracy. The general term which suggests the decision-making power based on technical expertise of the new professionals is "technocratic" (Beer, 1973:74–80; 1977a:30–34). By using it I do not mean to raise fears of a dictatorship of men in white coats. On the contrary, one of the more interesting features of this new influence is the way in which it has promoted the rise of a countervailing power in the form of the intergovernmental lobby (Farkas, 1971; Haider, 1974; Plant, 1975). By the intergovernmental lobby I mean the governors, mayors, county supervisors and other officeholders, usually elective, who exercise general responsibilities in state and local governments.

Like the new professionalism the intergovernmental lobby in some form can be found in other advanced countries. A term suitable for comparative use is needed. I propose the word "topocrat," from the Greek *topos*, meaning "place" or "locality," and *kratos*, meaning "authority"–and herewith thank its author, Roy Macridis, who responded to my request for a single, general and non-American term that would save me from having to say "state and local government officials" and the equivalent mouth-filling phrase when speaking of other countries.

In the United States these topocrats act in a representative capacity through their organizations, the National Governors Conference, the Council of State Governments, the United States Conference of Mayors, the National League of Cities, the National Association of Counties, the International City Management Association, and the National Legislative Conference, and through alliances of them, such as the "Big Six" during the initial enactment of general revenue sharing. But their most important front lies along the continual, almost day-to-day activity of individual officeholders and their agents, offering advice and pressing requests before the executive and legislative branches of the federal government.

To appreciate the significance of this activity we must remind ourselves how very recently state and local officeholders began to take an interest of this kind and degree in what goes on in Washington. Their sudden surge of activity dates from the mid-sixties and came as a response to the increase in federal programs spawned by the new professionalism. As the mayors and other executives of the governments through which these programs were carried out became aware of their value–their political value and their problem-solving promise–they developed a heightened interest in and increasing contact with federal policy making and administration. Their Washington activities grew and their national organizations headquartered there expanded in members, budgets and staffs. In this sense it was not the lobby that created the programs but the programs of the professional bureaucratic complex that created the intergovernmental lobby (Beer, 1976:163–71).

In time the intergovernmental lobby acquired sufficient political influence to initiate demands more closely reflecting its members' political interests. A major demand was for fewer federal strings on more federal money. Accordingly, in the past few years the mayors, governors, county executives and others have played a major role in bringing about a shift in the character of federal aid. In 1966 categorical aid accounted for 98 percent of federal aid. By 1975 it was down to 75 percent as a result of general revenue sharing and block grants (GCO, 1976:26).

In brief, then, I am saying that over the past generation and especially since the early sixties the technocratic tendencies of the new professionalism have called forth the topocratic tendencies of the intergovernmental lobby. If one asks whether the process has been centralizing or decentralizing the answer must be a bit complicated. It has been strongly centralizing insofar as the new programs have carried the technocratic perspectives formed at the federal level into the daily thought and action of state and local governments. It has been decentralizing in that these governments, as the administrative agents of the new programs, have often been able to adapt them to their own local purposes, an option that has been deliberately expanded by some loosening of federal strings. The process was centralizing in that it drew state and local office holders into direct contact with the federal government and decentralizing in that it has brought their topocratic perspectives to bear on federal policy making. The trend is not toward a centralized unitary system. Neither is there much sign of significantly greater autonomy for state or local governments. But whatever that balance may be, the interesting and important thing, it seems to me, is the way in which the polarity of representational federalism has been reasserted. The impact of technocrat and topocrat upon opposing levels of government is adding a new dimension in an old pattern to our system of national representation.

Comparative Perspectives

In advanced countries today, there is a good deal of concern over the great and growing size of the public sector of the economy (Beer, 1977b). In this article I have dealt with a parallel development in the United States: the growing influence of the public sector of the polity and specifically the emergence of new structures of representation within that sector. These structures reflect enduring traits of American federalism. They also resemble certain developments in other advanced countries and their study should therefore be assimilated to the comparative study of the contemporary welfare state.

That study is often focused on the conflicts and connections of function and territory, of center and periphery (Ashford, 1976a, 1976b; Tarrow, 1977). My similar concern with the interaction of social pluralism and territorial pluralism in the American experience suggests two broad questions for comparison and contrast with other countries.

First, the technocratic question. Professionalism extends its domains in all advanced countries and surely will continue to do so. The modern state at this late date is not likely to turn its back on science and technology. In these countries, as in the United States, there has been a surge of social spending since the 1960s. Professional, centrally directed hierarchies are the normal agencies of the programs financed by this spending.

But whom or what do these agencies represent? In their dealings with their local authorities they represent their central governments. To what extent do they also represent social and economic interests within the society and outside the public sector? In many welfare states, beginning as far back as the interwar years, organized interests developed relations with government departments so close and institutionalized as to constitute a system of functional representation. How far are the professional hierarchies of recent years dependencies of these corporatistic arrangements? How far are they veritably technocratic in the sense of generating their own initiatives from within the public sector and independently of ourside interests? In what ways, if any, is the social pluralism of modern society transmitted to the professional bureaucratic complex?

Second, the topocratic question. In spite of overpowering forces making for centralization, the modern state seems to be unable to do without territorial subunits. Nor are these mere branch offices of administration, but rather governmental units with a political capacity. Much has been said, but little done in any country about decentralizing power to them or enlarging their autonomy. They have been widely used, however, as vehicles for carrying out the programs of central governments and, at the same time, have taken an increasingly active role as agents of representation before, and indeed, within those central governments. In other countries, both unitary and federal, the equivalent of the American intergovernmental lobby has appeared and flourishes. These varieties of topocratic representation are many and complex and light up major contrasts among the different versions of the modern state.

As we will want to look into the relation of technocratic representation to older processes of pressure group politics and functional representation, we shall also ask what are the conflicts and connections between topocratic representation and the older structures of territorial representation. In the U.S., for instance, the political relations of mayors and members of Congress are one of the great unexplored mysteries.

Moreover, whom does the topocrat represent when he formulates the presumed needs of his government before central authorities? He will commonly speak on behalf of groups among his constituents. He may know and be responsive to them. He might even have been chosen by them in part because he was expected to be a good lobbyist in the national capital. Yet he is subject not only to their wishes, but also to an array of influences proceeding from his position as the agent of a bureaucratic and political body. This governmental position will affect and may dominate his representative role.

I can communicate my unease at these technocratic and topocratic dilutions of the popular will by saying that the new structures have a strong connotation of corporate rather than personal representation. They do add real strengths to the modern state. But this may be at some cost to free government.

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The Effect of Registration Laws on Voter Turnout*

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After the drastic relaxation of voter registration requirements in the 1960s, do present state laws keep people away from the polls? More specifically, which provisions have how much effect on what kinds of people? We have answered these questions with data from the Current Population Survey conducted by the Census Bureau in November 1972.

State registration laws reduced turnout in the 1972 presidential election by about nine percentage points. The impact of the laws was heaviest in the South and on less educated people of both races. Early deadlines for registration and limited registration office hours were the biggest impediments to turnout.

Contrary to expectations, changing these requirements would not substantially alter the character of the electorate. The voting population would be faintly less affluent and educated; the biggest difference would be a matter of one or two percentage points. In strictly political terms, the change would be even fainter—a gain for the Democrats of less than half a percent.

Most Americans, in order to vote, must establish their eligibility by registering prior to election day. This requirement makes voting a more difficult act than it otherwise would be. Not only must citizens care enough to go to the polls, but to register they must also make an earlier expenditure of time and energy. Indeed, registration is often more difficult than voting. It may require a longer journey, at a less convenient hour, to complete a more complicated procedure—and at a time when interest in the campaign is far from its peak.

This aspect of the American electoral system is unusual. In most democratic countries the government assumes responsibility for enrolling all citizens on a permanent, nationwide electoral register. This difference is widely considered a major cause of the low rate of voter turnout in the United States.

Previous research provided reasons for accepting the omnibus proposition that the state registration laws in force in 1960 made a difference in turnout that year. Two studies showed that much of the variation in voting levels among cities and states was explained by interstate differences in laws governing the registration of voters.[1] But little light was shed on *what* provisions had *how much* influence on the voting levels of *what kinds of people*. The sweeping changes in election laws since 1960 further reduced the contemporary relevance of these studies. Perhaps because of these uncertainties, some scholars, publicists, and politicians expressed doubts that registration laws in the 1970s have much to do with the number of Americans who go to the polls.[2] Others insist that the existing legal situation still "results in disenfranchisement on a massive scale."[3]

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[1] Stanley Kelley, Jr., Richard E. Ayres, and William G. Bowen, "Registration and Voting: Putting First Things First," *American Political Science Review*, 61 (June 1967), 359–79; and Jae-On Kim, John R. Petrocik, and Stephen N. Enokson, "Voter Turnout Among the American States: Systemic and Individual Components," *American Political Science Review*, 69 (March 1975), 107–23.

[2] Kevin P. Phillips and Paul H. Blackman, *Electoral Reform and Voter Participation* (Washington: American Enterprise Institute for Policy Research, 1975), p. 14; and V. Lance Tarrance, "The Vanishing Voter: A Look at Non-Voting as a Purposive Act," in *Voters, Primaries, and Parties*, ed. Jonathan Moore and Albert C. Pierce (Cambridge, Mass.: Harvard University Institute of Politics, 1976), pp. 11, 12. See also the minority views in Committee on Post Office and Civil Service, *Voter Registration by Postcard*, Senate Doc. 352, 93d Congress, 2d sess.

[3] Senator Gale McGee, *Congressional Record*, 13 March 1975, p. S6451. For other claims by politicians,

This debate underlies continuing attempts, at both state and federal levels, to ease further the burden of registration. President Carter's unsuccessful 1977 proposal for election-day registration was the latest example. In 1976 a bill to establish a scheme for nationwide post-card registration was justified by its congressional sponsors as a remedy for the "obstacle course" produced by existing state laws.[4] Scholars are interested not only in the accuracy of this claim, but in the more fundamental questions raised by the debate. Discerning variations in the impediments to voting posed by alternate registration provisions helps us understand the saliency to individuals of political participation. If apparently trivial differences in the prerequisites to casting a ballot have substantial effects on the probability of voting, what does this tell us about the relative costs and perceived benefits of going to the polls? How do individuals differ in the extent to which registration requirements impede their access to the ballot box? In attempting to answer these questions, we will be concerned not only with the extent to which registration provisions affect turnout, but also with the political consequences of relaxing the registration laws to make voting everywhere as easy as in the most permissive states. The last section of this article compares the actual presidential electorate in 1972 to the larger, hypothetical electorate that would be produced by easier registration.

In estimating the impact of registration laws, one is identifying the degree to which statewide legal provisions influence individuals. Previous researchers did not dispute the desirability of using individual-level data to study the problem. They used aggregate voting and demographic data because the existing academic surveys, with samples ranging from 1500 to 2300 respondents selected from fewer than 40 states, were an inadequate data base. In this study our data come from the Current Population Survey conducted by the Bureau of the Census in November 1972.[5] The sample is nearly 40 times larger than that used by the University of Michigan Center for Political Studies and includes respondents from all 50 states. We will describe our data sources in detail, after a discussion of recent changes in American suffrage laws.

State Suffrage Laws, 1960–73

Each state determines its own registration laws, subject only to certain limitations imposed by the U.S. Constitution, court decisions, and national legislation. Despite these limitations (many of which are quite recent), the states differ widely in the extent to which registration significantly increases the time and energy costs of voting. In some states registration is relatively easy; some have abolished this prerequisite altogether. In other states registration is relatively onerous.

Our examination of state laws begins in 1960 because that year's election provided the data for the two principal studies of the effect of these laws on voter turnout (see note 1). For our purposes, 11 different areas of legal regulation are identified.

1. *Poll taxes.* In 1960 some southern states imposed a tax as a condition for registration. A few states made the tax liability cumulative, i.e., before being allowed to register, one had to pay not only the current year's tax, but the tax for all earlier and unpaid years of his eligibility. The Twenty-fourth Amendment to the Constitution prohibited poll taxes in federal elections in 1964. Two years later the Supreme Court abolished the practice altogether.[6]

2. *Literacy tests.* Most common in the South, these ranged from requiring simple literacy to a demand that the applicant interpret a provision of the state or federal constitution. Through inequitable enforcement,

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see *Voter Registration by Postcard;* and the speeches by Senators McGee, Kennedy, Cranston, Williams, Montoya, and Humphrey on March 13, reported on pp. S6450–54 of the *Congressional Record.* Probably the most active and pervasive interest group working to loosen registration laws is the League of Women Voters. See, for example, *Administrative Obstacles to Voting,* Pub. No. 206 (Washington: League of Women Voters of the United States, 1972). The Yankelovich polling organization suggested that registration laws kept 26 million people from the polls in 1972. This figure is an extrapolation from the survey respondents who told interviewers that they would have voted if they had not had to register. "A Study of the Registration Process in the United States (The Registered and the Non Registered)" by Daniel Yankelovich, Inc. for The Student Vote, Inc., 1973, p. 23.

[4] Similar bills were introduced in 1971, 1973, and 1975. All of them foundered on opposition by Republicans and southern Democrats.

[5] A description of sampling procedures and other technical matters is in Bureau of the Census, "Voting and Registration in the Election of November 1972," *Current Population Reports, Population Characteristics,* October 1973, Series P–20, No. 253. We obtained the data tape from the Inter-University Consortium for Political Research. Neither the ICPR nor the Census Bureau is responsible for our tabulations and analyses.

[6] *Harper* v. *Virginia State Board of Elections,* 383 U.S. 663 (1966).

this was the principal administrative means of disenfranchising southern blacks. Literacy tests were suspended in much of the South by the Voting Rights Act of 1965. They were abolished everywhere by the Voting Rights Act Amendments (VRAA) of 1970.

3. *Permanent or periodic registration.* In 1960 a number of states required potential voters to re-register at intervals of as little as one year. By 1972, this practice was followed in only two states which required re-registration at ten-year intervals. A few states allowed re-registration at the discretion of local officials. This option has been exercised in a couple of southern states, presumably as a means of reducing black turnout.[7]

4. *Purging for nonvoting.* Akin to periodic registration is the requirement that people who have not voted within a stated time be dropped from the registration rolls. The period ranges from two to eight years. Some states have no purging provision at all.

5. *Residency requirements.* In the early 1960s, 38 states required at least a year's residence in the state before one could register. The 1970 Voting Rights Act Amendments in effect imposed a maximum 30-day residency requirement for presidential elections. They also permitted new residents to vote in their previous state either in person or by absentee ballot. In March 1972 the Supreme Court, in *Dunn* v. *Blumstein,* struck down Tennessee's one-year requirement.[8] As a result of this decision, residency requirements for other elections were restricted. Although the Court did not impose a new limit, its decision was interpreted as strongly suggesting a maximum residency requirement of 30 days before the election.

6. *Closing date.* This is the last day one can register before the election—the date registration is closed. The 1970 VRAA mandated a closing date of at most 30 days before presidential elections. The Court's decision in *Dunn* v. *Blumstein* said that "30 days appears to be an ample period of time" in other elections. But a year later, the Court permitted Arizona and Georgia to retain their 50-day closing dates.[9] It is easy to confuse closing dates and residency requirements, and the two are, of course, interdependent.

7. *Regular office hours.* Most states require registration offices to remain open Monday through Friday during normal business hours. Other states either impose less stringent schedules or are silent on this subject. Thus offices in some states may be open only a few hours on some days of the week, drastically reducing opportunities for registration.

8. *Evening and Saturday registration.* Recognizing that most people work during the day from Monday to Friday, many states require that registration offices be open after normal working hours, on Saturday, or both.

9. *County, city, or neighborhood offices.* In some states potential voters can register only at the county seat. Elsewhere registration offices are located in each city. Most convenient is neighborhood registration, usually in firehouses, libraries, and the like.

10. *Deputy registrars.* Some states authorize deputizing ordinary citizens to register voters. Deputy registrars go from house to house, set up tables in shopping centers, and so on. Such provisions are often exploited by political parties and interest groups.

11. *Absentee registration.* The 1970 VRAA required all states to permit absentee registration in presidential elections solely on grounds of absence. This is a minimum standard. Some states permit absentee registration for various other reasons, and a few authorize it for anyone. Federal postcard registration would authorize nationwide absentee registration. A major issue in congressional debate has been whether postcards should be mailed to everyone, or whether the individual would have to obtain a postcard to mail in. The latter alternative resembles the system used by five states in 1972.

Previous Research

For a considerable period after the advent of survey research on voting behavior, systematic evidence about the effects of registration laws was scarce and limited. The authors of *The American Voter* reported that legal considerations seemed unrelated to voting levels in the North, but reduced turnout in the South by about nine percent.[10] Rough estimates about the number of people kept from the polls by residency requirements were commonplace. Perhaps the best-known figure was provided by the President's Commission on Registration and Voting Participation, which claimed that eight million Americans had been unable to vote in 1960 because of residency requirements.[11]

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[7] Alan Reitman and Robert B. Davidson, *The Election Process: Voting Laws and Procedures* (Dobbs Ferry, N.Y.: Oceana Publications, 1972), pp. 33–34.

[8] *Dunn* v. *Blumstein,* 405 U.S. 330 (1972).

[9] *Marston* v. *Lewis,* 410 U.S. 679 (1973); and *Burns* v. *Fortson,* 410 U.S. 686 (1973).

[10] Angus Campbell et al., *The American Voter* (New York: John Wiley, 1960), p. 277. The only legal variables studied were residency requirements and literacy tests in the North, and poll taxes and residency in the South (p. 269). Legal variables seemed to have more impact on blacks in the South, but the small number of cases precluded controls for education (pp. 279–80). For a similar conclusion, see

The paucity of detailed evidence reflected the considerable difficulties of research. Several aspects of registration can be handled in various ways; thus numerous combinations of legal provisions are possible. The combinations are far from random, however. States with restrictive procedures in one aspect of registration are likely to be more demanding in other respects. Moreover, individuals who vote less, such as uneducated people and southerners, are more likely to be found in states with restrictive laws.

The first scholars to tackle these problems were Stanley Kelley, Jr., and his associates.[12] Acknowledging that registration is an inescapable prerequisite to voting, they set out to explain variations in registration among 104 cities in 1960. By means of multiple regression they estimated the differences in registration related to demographic characteristics and to registration laws, all measured on a city-wide basis.[13] They concluded that "local differences in the turnout for elections are to a large extent related to local differences in rates of registration, and these in turn reflect to a considerable degree local differences in the rules governing, and arrangements for handling, the registering of voters."[14]

One drawback of explaining variance at the aggregate level is an inevitable inflation of the importance of the laws.[15] Cities (and states) are much more similar with respect to demographic than legal variables. The latter often are dichotomous, whereas aggregate demographic differences represent points along a continuum that rarely are very far apart. Since there is not very much state-to-state variation in demographic variables, they cannot explain much of the variance in turnout. This is not the case with legal variables, which do vary dramatically from state to state. As a result, this approach exaggerates the importance of the laws.[16]

We do not consider "explaining variance" as interesting as establishing the extent to which particular provisions increase or decrease turnout. This would be a more satisfactory estimate of the effect that the laws have on turnout. It would also allow one to forecast the consequences of proposed changes in the laws. Moreover, previous studies did not consider possible interaction effects between particular provisions and specific groups in the population. They assumed that whatever the effect of a provision, it fell equally on everybody.[17]

Quite apart from their merits as sources of insight into turnout in 1960, these studies have lost much of their contemporary relevance because of subsequent sweeping changes in state and federal laws. Poll taxes and literacy tests have been abolished; periodic registration has vanished except for ten-year re-registration in Arizona and South Carolina; residency requirements are measured in days rather than years; closing dates are closer to election day; and there are minimum national standards for absentee registration. With the present range of interstate variation so drastically reduced, do

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Lester Milbrath, "Political Participation in the States," in *Politics in the American States,* ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown, 1965), p. 47.

[11] Report of the President's Commission on Registration and Voting Participation, November 1963, p. 13. Campbell and his associates said that residency requirements "prevented at least 3 percent of our respondents from voting" (*The American Voter,* p. 90n.).

[12] Kelley et al., "Registration and Voting."

[13] Some of the legal variables were assigned numerical values on the basis of the researchers' "guesses" about their impact on registration (Kelley et al., 366n.).

[14] Ibid., pp. 373–74. Another study concluded that registration laws explained a good deal of the variations in turnout among the states in 1960. See Kim et al., "Voter Turnout Among the American States."

[15] Kelley et al. recognized that "inter-city analysis is less appropriate for an examination of the effects on registration of individual characteristics such as sex, age, education, and income, which can be better studied by treating the individual as the unit of observation" (p. 362).

[16] Cf. J. Johnston, *Econometric Methods* (New York: Harper & Row, 1974), pp. 34–35, 129.

[17] Another unsatisfactory approach is asking respondents why they were not registered. Answers to this question are in our data set. We tabulated the registration laws in each respondent's state of residence with the reasons offered for failure to register. The result is disillusioning for anyone who believes that human motivation can be discerned by asking people to explain their behavior. We found no relationship between the objective permissiveness of the laws and the proportion of respondents who said they were unregistered because they could not satisfy the residency requirement or had been unable to register. For example, in states where registration offices were required to remain open at night or on Saturday, 15 percent of those who were not registered attributed this condition to their inability to register, compared to 14 percent who gave this excuse where such extended hours were not required.

the remaining differences still have any impact on turnout?[18]

Another major change since the early 1960s is the massive reduction of barriers to black voting in the South. As long as blacks were prevented from registering by intimidation and maladministration of the laws, it was difficult to sort out the extent to which turnout was related to intimidation and to race, education, region, and restrictive laws. By enfranchising millions of southern blacks, the federal Voting Rights Act of 1965 removed one cause of the strong relationships between turnout and race, region, and education.[19] In addition to reducing the importance of race, region, and education as independent variables, the Voting Rights Act may well have reduced the interaction effect of these factors. That is, the Act may have reduced the likelihood that any given provision has a different impact on different people, instead of falling with equal effect on the entire population. Thus the findings that registration laws affect turnout only in the South may be as obsolete as Jim Crow laws.

Data Sources

We used survey data on individual demographic characteristics and turnout in the 1972 general election. This information was gathered in the Current Population Survey conducted by the Bureau of the Census in November 1972. Beginning in 1964, the November Current Population Survey in each even-numbered year has included questions on registration and turnout, as well as the customary demographic and employment data.[20]

Several characteristics of this data collection make it well-suited for our purposes. Most obvious is the sheer size of the sample: 93,339 respondents were interviewed. Weighting to achieve a representative sample of the civilian noninstitutional population yields 136,203 cases for analysis.[21] Other aspects of the sampling procedure are perhaps more important. The sample includes all 50 states and the District of Columbia; only three states have less than 200 actual respondents. In contrast, the Michigan Center for Political Studies 1972 National Election Study has no respondents from 13 states, including some with the least restrictive registration laws, and 45 or fewer respondents in an additional 13 states.[22] Moreover, the Census survey uses a very large number of sampling units. The 1972 survey was drawn from 461 primary sampling units (PSUs), compared to 74 PSUs in the Michigan sample.[23]

Taken together, these properties of our sample mean that the sampling variability is very small and thus the resulting population estimates are unusually precise. Moreover, geographic categorization of the respondents need not be limited to gross regional divisions. Because there is a relatively large number of respondents from every state, the effects of state laws can be estimated precisely.[24]

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[18] One recent study comparing turnout in the 1960 and 1972 elections is Orley Ashenfelter and Stanley Kelley, Jr., "Determinants of Participation in Presidential Elections," *Law and Economics*, 18 (March 1976), 695–733. This article uses data on 925 cases drawn from the University of Michigan Center for Political Studies' national sample survey of the 1972 election. The estimates of the effect of registration laws are imprecise, perhaps because of the limitations of the Michigan sample for this purpose (see p. 26).

[19] In 1962, five percent of the black voting-age population of Mississippi and 13 percent in Alabama were registered to vote. Ten years later the figures were 59 percent and 55 percent. Sources: for 1962, Neal R. Peirce, *The Deep South States of America* (New York: W. W. Norton, 1974); for 1972, John Lewis and Archie E. Allen, "Black Voter Registration Efforts in the South," *Notre Dame Lawyer*, 48 (October 1972), 119.

[20] Data on turnout come from replies to this item: "This month we have some questions about whether people voted in the November 7th Presidential Election. Did (you) (this person) vote in the election held on November 7th?" For almost half the sample turnout data were provided by the respondent for another family member. Such second-hand interviewing raises the possibility of misreporting, as Edward R. Tufte suggests ("Political Statistics for the United States: Observations on Some Major Data Sources," *American Political Science Review*, 71 [March 1977], 306). This fear seems unjustified, however. Respondents who reported their own vote had a turnout rate of 66.7 percent. People in the sample for whom voting was reported by another family member had a turnout rate of 67.7 percent. We also estimated the effect of second-hand reporting by entering a dummy variable (1 = second-hand reporting) in the probit equation summarized in Table 2. The resulting estimate was .007 with a standard error of .010. This number is substantially zero, and no change occurred in the coefficients reported in Table 2. In short, we feel justified in assuming that any errors in the reporting of turnout (whether first-hand or second-hand reports) are uncorrelated with registration provisions.

[21] The weighting procedure is described in Bureau of the Census, "Documentation of the Annual Demographic File," 1975, Appendix A, pp. 4–12.

[22] *CPS 1972 American National Election Study* (Ann Arbor, Michigan: Center for Political Studies, 1973), Variable 4.

[23] Bureau of the Census, "Voting and Registration in the Election of November 1972," p. 13. For additional information on sampling procedures see Bureau of the Census, *Current Population Survey: A Report on Methodology*, Technical Paper No. 7, U.S. Department of Commerce, 1963; and "Documentation of the Annual Demographic File," Appendix A; and Center for Political Studies, *1972 Study*, p. xii.

We deleted noncitizens from the sample, thus losing 2,444 actual respondents (the weighted N = 3,522). We also excluded from analysis all cases where the respondent did not know whether a vote had been cast, or where this information was not ascertained by the interviewer. This reduced the sample by 2,790 actual respondents, or a weighted N of 4,099. These modifications bring us to 88,105 actual respondents, or a weighted N of 128,582. A fuller description of our sample and comparison with other data sets is presented in Appendix A.

Data on Registration Provisions. Obtaining information about each state's voting laws proved to be more difficult than we had expected. Several publications contain lists of state registration laws for the 1972 general election.[25] Unfortunately, they were all compiled before the Court handed down its ruling in *Dunn* v. *Blumstein*.[26] Thus they do not take account of that decision, nor of the numerous state-level responses to it that changed residency requirements or closing dates for the November election.

We tried to obtain the information by sending a one-page questionnaire to each state's chief election official. They were quick to respond to our inquiry; 41 of them replied within a few weeks, although not always with accurate responses. A check with the statutes themselves indicated that over a third of the returned questionnaires had at least one error. We think this reflects the time that had passed since the 1972 election, changes both in personnel and in state laws, and possible confusion between laws governing presidential and other elections.

We finally gathered the information directly from the Library of the Boalt School of Law on the Berkeley Campus of the University of California. This material for each state includes not only the state code, but all relevant state and federal court decisions, and also state attorney general opinions if federal legislation or court decisions altered the application of state statutes.

Our coding scheme for the state laws is described in Appendix B. Here we discuss some of the ways we reconciled the complexities of real life to the more simplistic world of quantitative analysis. One set of problems along these lines pertains to states that mandated different procedures for different areas, usually on the basis of population. For example, Iowa, Minnesota, Missouri, Ohio, and Wisconsin exempted their less populous counties from registration requirements. Since in each case only a small fraction of the state population was excluded and we could not identify our respondents' counties, we ignored this deviation from the laws governing most of the states' residents.

A second set of problems was encountered with the differences between state laws that required a particular practice, or allowed it, or forbade it. We have no way of knowing the extent to which permissive state legislation actually is exploited at the local level. For example, many states authorize the appointment of deputy registrars. This is not the same thing as requiring the deputizing of a set number of registrars to go from door to door or sit at tables in shopping centers and student unions. Lacking knowledge of the extent to which registrars are appointed (not to mention the vigor with which they do their work), we could only distinguish between states that permit deputy registrars and those that do not. On the question of regular hours for registration offices, we classified as "irregular" all states that prescribed any schedule less than regular business hours five days a week. We combined states that required such schedules and those that did not legislate on the issue when our preliminary analysis showed no differences in turnout. States that required evening and/or Saturday office hours were separated from those that did not. (A token Saturday or two was not enough.)

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[24] Information on sampling error in the Census survey and the procedures for adjusting the standard error of estimates for groups of states below the regional level can be found in "Documentation of the Annual Demographic File," Appendix A, pp. 13–45, 62–66; and "Voting and Registration in the Election of November 1972," pp. 13–16.

[25] U.S. Senate, Committee on Post Office and Civil Service, as reported in the *Congressional Record*, 8 March 1972, pp. 7530–31; Robert Thornton, "Election Legislation," in *The Book of the States, 1972–73,* 19 (Lexington, Ky.: Council of State Governments, 1972), pp. 25–30; Reitman and Davidson, *The Election Process;* and League of Women Voters Education Fund, "Registration and Absentee Voting Procedures by State, 1972" (Washington, D.C., 1972).

[26] One exception is the list compiled by the Congressional Research Service. Printed just after *Dunn* v. *Blumstein,* this publication claims that every state's residency requirement is governed by the 1970 VRAA for presidential elections and by *Dunn* for other elections. This blanket description proved to be wide of the mark. See Library of Congress, Congressional Research Service, *Election Laws of the Fifty States and the District of Columbia,* April 1972.

In short, a great deal is often left up to local officials and the initiative of local interest groups and parties.[27] In view of these large areas of discretion, we acknowledge that our data do not necessarily describe the reality of administration and enforcement at the local level.[28] They do describe state-level enactments, which still vary considerably, as Table 1 shows. (Other specific coding decisions are described in the footnotes to Table 1.)

Estimating the Impact of Registration Laws on Turnout

Two general propositions about the effect of registration requirements on turnout are suggested by the literature on the personal costs of voting: (1) The more time and energy required to vote, the lower the probability that an individual will vote.[29] (2) The costs of voting affect some people more than others; registration laws have their greatest impact on people with less education.[30]

We think that there are two explanations for the second proposition. First, more than any other personal characteristic, education increases political interest. More exposure to school produces more information about public issues and greater capacity to understand them. Therefore, better-educated people are more likely to be interested enough to overcome the inconveniences of limited office hours and earlier deadlines. Second, the bureaucratic skills acquired in school reduce the difficulty of overcoming these hurdles. In other words, the likelihood that an individual will vote is not merely a behavioral manifestation of certain individual interests. It also reflects the ease with which these individual predilections can be expressed in action. More education produces both a bigger incentive to jump the hurdle and a lower hurdle.

As Table 1 shows, states that have restrictive requirements in one aspect of registration are likely to have restrictive provisions in other aspects. Moreover, demographic variables such as race, region, age, and education are correlated both with the registration provisions and with turnout. Better-educated people, northerners, and whites are more likely to vote. They also tend to live in states with more permissive registration laws. To estimate the effect on turnout of a particular registration requirement, the other legal provisions and the demographic variables must be held constant. Perhaps the most common technique for doing this is ordinary least squares (OLS) multiple regression. But because our dependent variable (turnout) is dichotomous, our data do not meet the usual OLS assumptions. We used probit analysis rather than multiple regression to estimate the effect of registration laws. (See Appendix C for a comparison of probit and OLS.) Like OLS, probit allows us to estimate the effect of a single independent variable on the dependent variable while holding constant all other independent variables in the equation. In the discussion that follows all estimates have been converted to probabilities.

Because of the tremendous cost of estimating a series of equations using all respondents in our sample, we took a subsample for the probit analysis.[31] We designed this subsample to maximize the variation among registration requirements, thus improving the estimates of their impact on turnout.[32] We ignored the weighting factor and, within each state, randomly selected actual respondents. For most states, we chose about 150 respondents per state.[33] (Alaska, Nevada, and Vermont each had less than 150 respondents, all of whom

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[27] In Arizona, for example, a recent county registrar of voters required prospective deputy registrars to take a rigorously graded examination. His successor brought about a substantial increase in the number of registrars by relaxing these standards. (We are indebted to Bruce B. Mason of Arizona State University for this information.)

[28] The frequent erroneous responses in our mail survey of state election officials suggest that it would be virtually impossible to gather accurate information on how state laws are administered in the nation's thousands of counties and cities.

[29] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), pp. 255–56, 273.

[30] Raymond E. Wolfinger and Steven J. Rosenstone, *Who Votes?* Forthcoming.

[31] Using the entire sample would have required about 50 *minutes* of computer time for each probit equation estimated. Although we did not use all respondents for the crucial stage of our analysis, it should be noted that a national sample of such magnitude is necessary to provide adequate state-by-state subsamples.

[32] Other things being equal, increasing the variance decreases the standard error of the estimate. See Henri Theil, *Principles of Econometrics* (New York: John Wiley, 1971), pp. 90–91; and D. J. Finney, *Probit Analysis*, 3d ed. (Cambridge: Cambridge University Press, 1971), pp. 33–37.

[33] We excluded respondents in the District of Columbia from all data analyses reported hereafter. Since their franchise is limited and an inordinate number of District residents maintain legal voting residences elsewhere, we thought we would avoid unnecessary complications.

Table 1. State Turnout and Registration Laws–1972[a]

State	Turnout[b]	Residency Requirement	Closing Date–Days Before Election	Where to Register[c]	Deputy Registrars Allowed	Registration Office Open 40 Hours a Week	Evening and/or Saturday Registration Required	Absentee Registration[d]	Years of Non-Voting Before Purging From Rolls
Utah	69.4	31	11	C		X	X	X	4
South Dakota	68.9	15	15	T	X	X	X	X	4
Minnesota	68.9	30	20	T	X	X	X	X	4
Connecticut	68.6	7	24	P	X		X	X	None
North Dakota	68.2	10	No Registration[e]						
Montana	68.1	30	30	P	X	X		X	2
New Hampshire	65.5	30	9	T				X	None
Massachusetts	65.2	29	28	T	X	X	X	X[f]	None
Washington	65.1	31	31	P	X	X			2.5
Illinois	64.2	30	28	P	X	X	X	U	4
Wyoming	64.1	30	15	T		X		X	2
Idaho	63.8	3	3	P	X	X		U	8
Iowa	63.6	10	10	P	X	X	X	X	4
Rhode Island	63.5	30	30	T	X	X	X	X	5
California	63.0	7	30	P	X	X	X	X	2
Delaware	63.0	17	17	P	X		X		4
West Virginia	62.7	30	29	C		X		X	2.4
Wisconsin	62.5	1	13/20[g]	T	X	X		X	2
Oregon	62.5	1	31	P	X	X		X	None
Vermont	62.3	30	3	T					None
Maine	62.2	30	8[h]	T	X		X	X	None
New Jersey	62.0	30	40	T	X	X	X	X	4
Indiana	61.2	30	29	P	X	X		X	2
Colorado	60.8	29	32	P		X		X	2
Michigan	60.6	30	30	T	X	X	X	X	4
Kansas	59.3	1	20	C	X	X	X	X	2
New York	59.1	30	24	P	X	X		X	2
New Mexico	58.3	30	42	P	X	X		X	2
Ohio	58.1	14	13	C	X	X		X	2
Missouri	57.8	30	28	T	X	X		X	4[h]
Oklahoma	57.2	15	11	P	X	X			2/4[i]
Pennsylvania	56.6	30	31	T	X	X		X	2
Nebraska	56.3	2	11	C	X	X		X	4
Hawaii	54.2	26	26	P	X	X		U	2
Nevada	51.9	30	38	C	X	X	X		2
Florida	51.3	30	31	T	X	X		X	2

Table 1. (continued)

State	Turnout[b]	Residency Requirement	Closing Date–Days Before Election	Where to Register[c]	Deputy Registrars Allowed	Registration Office Open 40 Hours a Week	Evening and/or Saturday Registration Required	Absentee Registration[d]	Years of Non-Voting Before Purging From Rolls
Maryland	51.1	30	29	T	X			X	5
Arizona	49.0	30	50	P	X	X		X	2
Alaska	48.9	30	30	P		X		U	4
Kentucky	48.5	30	30	T	X	X			2
Arkansas	47.9	30	20	C		X		X	4
Texas	46.2	61	30	C	X	X		U	3
Virginia	45.9	30	30	T	X				None
Mississippi	45.1	30	30	C		X			None
Louisiana	44.6	30	30	C	X	X	X		4
Tennessee	43.7	50	30	C	X		X	X	4
Alabama	43.6	11	11	C				X	None
North Carolina	43.6	4	29	P		X			4
South Carolina	38.7	30	31	C	X	X			4
Georgia	38.1	30	50	C	X	X		X	3

[a] See Appendix B for sources and coding procedures.

[b] Percentage of *citizens* of voting age who cast a ballot in the presidential election. Source: Bureau of the Census, "Language Minority, Illiteracy, and Voting Data Used in Making Determinations for the Voting Rights Act Amendments of 1975 (Public Law 94–73)," *Current Population Reports,* Series P–25, No. 627; and letter from Gilbert R. Felton, Bureau of the Census, December 29, 1976.

[c] C = county only; T = town or city; P = precinct or neighborhood.

[d] U = universal absentee registration; X = permitted for absence and/or illness.

[e] Closing date was coded "0." Where to register was coded "P." Deputy registrars was coded "allowed." Registration office hours was coded "open 40 hours a week." Evening and/or Saturday registration was coded "required." Absentee registration was coded "universal." Years of non-voting before purging from rolls was coded "none."

[f] Only permitted if absent from county, but residing within the state, and then not by mail.

[g] Respondents living in the central cities of SMSAs under one million persons were coded "20." All other respondents were coded "13."

[h] Some intrastate variation which cannot be specifically coded.

[i] Respondents living in the central cities and suburbs of SMSAs under one million persons were coded "4." All other respondents were coded "2."

were selected.) Seven states with unusual combinations of registration provisions were oversampled to increase further the variation in the independent variables. Alabama, Arkansas, Georgia, Idaho, Louisiana, and Utah were oversampled by 50 percent; and all 202 respondents from North Dakota were included. The resulting subsample of 7,936 actual respondents was used in all subsequent analyses. In no way did we bias our estimates of the effects of the registration laws by using this subsample instead of the full sample. The only statistical "cost" is a slight increase in the standard error for each estimate.[34]

The demographic variables in our equation were education, age, and region. We also included squared terms for education and age to provide a better fit of the nonlinear relationship between these variables and turnout. When these variables are controlled, race does not have an independent effect on turnout.

Four registration provisions had a consequential impact on the probability of voting and were therefore included in the final equation (Equation II): (a) closing date; (b) regular hours for registration offices; (c) offices open evenings and/or Saturdays; and (d) the availability of absentee registration. Two other variables were included in the final equation because they had a small positive correlation with both the statutes and with turnout: the hours the polls were open on election day and a concurrent gubernatorial election in the state.[35] The estimated parameters of our final equation (Equation II) are in Table 2.

The effect of the registration provisions on the probability of *individuals* voting is summarized in Table 3. The provision with the largest impact is the closing date. Depending on one's probability of otherwise voting, a 30-day closing date decreased the likelihood of voting by 3 to 9 percentage points.[36] A 50-day closing date (in effect in Arizona and Georgia) lowered the probability of voting by about 17 percent for those with a 40 to 60 percent chance of going to the polls.

Variations in some other provisions also affected turnout. Irregular registration office hours (less than 40 hours a week) lowered by 2 to 4 percentage points the probability that a person would vote. Offices closed on Saturdays and in the evening decreased by 2 to 6 percent the probability of voting. In states that did not allow any form of absentee registration the chances of voting were 2 to 4 percent lower.

Two contextual variables other than the registration laws had an independent effect on turnout. Longer voting hours increased turnout a bit. Keeping the polls open for 14 hours increased the probability of an individual voting from 1 to 3 percent. A concurrent gubernatorial election raised turnout from 1 to 2 percent. On the other hand, a concurrent senatorial contest had no discernible impact on turnout. This difference between elections for senator and governor reflects the latter office's greater capacity to generate organizational resources for get-out-the-vote drives.[37]

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[34] See Theil, *Principles of Econometrics*, Ch. 8. All probit equations were estimated on the subsample of 7,936 actual respondents. While the case weight provided by the Census Bureau should be applied for descriptive purposes (e.g. cross tabulations), it is not appropriate to apply it for purposes of estimation. See Gary G. Koch, Daniel H. Freeman, Jr., and Jean L. Freeman, "Strategies in the Multivariate Analysis of Data from Complex Surveys," *International Statistical Review*, 43 (April 1975), 59–78; and Richard D. Porter, "On the Use of Survey Sample Weights in the Linear Model," *Annals of Economic and Social Measurement*, 2 (June 1973), 141–158.

[35] Our initial analysis also included these variables: (a) the number of days before the election an absentee ballot had to be obtained; (b) whether the absentee ballot had to be notarized; and (c) whether voting machines were used. These variables had less than a one percent impact on the probability of voting and their coefficients were not statistically significant. We deleted them after this early stage of the analysis.

In addition, estimates were made for nonlinear functions of the residency requirement and closing date. This did not appreciably improve the fit. We also estimated interaction terms between each provision and education, between deputy registrars and county registration, and between deputy registrars and irregular office hours. All were insignificant.

Finally, we examined the independent effect on turnout of income and party competition. Poorer people, of course, are less likely to vote, and party competition is often said to result in higher turnout. Thus any estimates of the relationship between registration laws and turnout that omitted consideration of these two variables might lead to a spurious conclusion.

We used the following procedure to determine whether these two variables in the error term should be included in the analysis. For one of these variables to bias the coefficient of a particular registration provision (which we will call X_1), two conditions had to be met: After controlling for the other independent demographic variables, the variable in question had to be correlated both with X_1 and with turnout. If either of these conditions were not met (i.e., if one of the partial correlations was zero), then the variable could safely be left out of the analysis without biasing the estimated coefficient for variable X_1.

Using this procedure, we found that income was not correlated with any registration provision, once education, region, and age had been controlled. The average magnitude of the partial correlations of the registration provisions was .029. The partial correlation between competition and turnout was –.009. Therefore, excluding these variables from Equation II does not bias the estimated coefficients for the registration laws. The measure of party competition we used was adopted from Austin Ranney, "Parties in State Politics," in *Politics in the American States*, 2d ed., ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown, 1972), p. 87.

[36] As explained in Appendix C, the estimated effect of an independent variable in probit analysis depends on the probability that the individual would otherwise vote.

Table 2. Equation II
Estimates of the Effect of
Demographic Variables and Registration Laws
on Turnout in 1972[a]

Variable	Probit Estimate	Standard Error
(Constant)	−2.7001	.2410
Education	.1847	.0120
Education squared	.0120	.0050
Age	.0707	.0045
Age squared	−.0006	.0001
Region	−.1371	.0413
Closing date	−.0073	.0015
Irregular office hours	−.1005	.0438
Open evening and/or Saturday	.1253	.0345
No absentee registration	−.0909	.0403
Hours polls open	.0336	.0159
Gubernatorial election	.0634	.0338

Number of cases = 7,936
Percentage of cases correctly predicted = 71.4
Log of the likelihood function = −4445.63
−2 times the log likelihood ratio = 1154.66
Degrees of freedom = 11

[a]Estimates for the variables deleted from this equation are given in Appendix D.

Southerners were somewhat less likely to vote than northerners even when education, age, and the registration laws were controlled. The regional gap varied between 2 and 6 percent. In other words, the continued lower turnout in the South is not fully explained by its demographic characteristics nor by its more restrictive registration laws.

We could find no evidence to suggest that in 1972 this regional gap was due to coercion of blacks in the South. An interaction term for southern blacks was included in the analysis. Interaction terms also were entered for southern blacks with each of the registration provisions. The estimated coefficients for these variables were neither substantively nor statistically significant. Southern blacks were no less likely to vote than one would expect on the basis of their age, education, and region. Voter registration provisions in 1972 did not have an added impact on southern blacks over and above this impact on white southerners with the same age and education.

Registration Law Reform and Nationwide Turnout

The estimates described above measure the impact of registration provisions on the probability of individuals voting in the 1972 general election. The question remains what the effect

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[37]For the evidence, see Wolfinger and Rosenstone, *Who Votes?*

Table 3. The Effect of Registration Requirements on the Likelihood of an Individual Voting in the 1972 Election[a]

Probability of an Individual Voting (%)	30-Day Closing Date	Irregular Office Hours	No Saturday or Evening Registration	No Absentee Registration
20	−6.7	−2.9	−3.8	−2.6
30	−8.1	−3.6	−4.7	−3.2
40	−8.7	−3.9	−5.1	−3.5
50	−8.7	−4.0	−5.5	−3.6
60	−8.2	−3.8	−4.9	−3.4
70	−8.1	−3.4	−3.4	−3.1
80	−5.6	−2.9	−3.4	−2.4
90	−3.3	−1.7	−2.1	−1.5

[a]The effect on turnout of a registration provision depends on the probability that an individual would otherwise vote. The cell entry is the estimated effect of the provision on an individual with the specified probability of voting. For example, a person who was otherwise 40 percent likely to vote was 8.7 percent less likely to do so if he lived in a state with a 30-day closing date. A person who was 90 percent likely to vote was only 3.3 percent less likely to do so as a result of a 30-day closing date. These estimates were derived by using the cumulative normal distribution to evaluate the probit estimates in Table 2.

would be on the level of national turnout if certain changes in the laws were adopted in every state. This cannot be learned directly from the probit estimates alone. For example, if a certain provision had a coefficient of –.10 but affected only 15 percent of the population, changing it would not have as much impact on aggregate turnout as would changing a provision that had a coefficient of –.07 but affected half the population.

To estimate the national electoral consequences of changes in the laws, we reweighted the subsample to be a representative sample of the civilian, citizen, voting-age population, excluding the District of Columbia.[38] Using Equation II, for each respondent we predicted what turnout would be if we assumed that certain registration provisions were enacted into law in every state. In each case, the projected provision was the more permissive alternative already used in some states. In other words, for each provision we assumed that every state had adopted the most permissive alternative in force in at least one state in 1972. From this estimate we subtracted the predicted turnout given the provisions as they actually existed in 1972. Summing across respondents yields an estimate of the projected aggregate change in turnout.[39]

Table 4 presents these projected increases in turnout for the country as a whole and for various demographic groups. The entries in each column represent the projected increase in turnout for the indicated group if the provision listed at the head of the column were adopted nationwide. The 1972 data do not allow us to estimate directly the increase in turnout that would result from allowing citizens to register *at the polls* on election day or from abolishing registration altogether. The most liberal reforms whose consequences we can estimate can be simulated by setting the registration provisions to the most permissive value existing in 1972. Therefore the "Total" column shows the total projected increase that would result from nationwide adoption of the following provisions:

a. eliminating the closing date;

b. opening registration offices during the 40 hour work week;

c. opening registration offices in the evening and/or on Saturday;

d. permitting absentee registration for the sick, disabled, and absent.

In short, Table 4 permits us to answer the questions about the impact of registration laws most interesting to policy makers: what will be the consequences, for what groups, of specified changes in the registration laws?

If all states adopted the provisions listed above, turnout would increase by approximately 9.1 percentage points.[40] In 1972, with a national potential electorate of about 134 million, this percentage translates into a projection that an additional 12.2 million people would have voted as a result of the indicated changes in the registration laws. We will decompose this aggregate projection in two ways: (1) To describe those changes that would have the greatest effect and those having relatively little impact; (2) to describe the types of people most affected by the changes.

As we said earlier, our findings indicate that relaxing existing provisions in some areas of registration would have no appreciable effect on turnout. In four areas, more permissive provisions would do little to increase voting.

> 1. *Residency Requirements.* In 1972, only two states had effective residency requirements of more than 31 days. Tennessee law produced a *de facto* 50-day requirement. In Texas, it was 61 days. Moreover, the 1970 VRAA required absentee voting in presidential elections for people who had recently moved. Our check of election codes indicates that every state had adopted such a provision by 1972. The result was that relatively few people were still affected by residency requirements. Our estimates indicate that the probability of voting in the

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[38] This reweighting was accomplished by multiplying, for each case, the original weight by the inverse of the sub-sampling proportion for the state from which the case was selected. This yields a reweighted N of 126,591. This number is slighty less than the weighted N mainly because respondents from the District of Columbia were deleted from the probit analysis and secondarily because of the random selection of cases in the sub-sampling procedure.

[39] For each respondent, Equation II was used to compute a probit estimate which was then converted to a probability by evaluating that number on the cumulative standard normal distribution. (The International Mathematical and Statistical Library includes a fortran subroutine which will compute this value.) The same procedure was followed, altering the values for the variables in Equation II to simulate changes in the registration provisions. For each respondent these two probabilities were subtracted. The arithmetic mean of these individual probabilities is the estimated aggregate percentage.

[40] The standard error of this estimate is 2.1. Thus there is only a 5 percent chance that the true coefficient is less than 4.9 percent or more than 13.3 percent.

1972 general election was not affected by existing residency requirements anywhere. Further relaxation of residency requirements would produce no appreciable increase in voting. This represents a substantial shift from conclusions about the effect of residency requirements in 1960, and doubtless reflects the enormous legal changes since then. Present requirements, in combination with provisions for absentee voting by new residents, have made residency requirements relatively unobtrusive.

2. *Deputy Registrars.* With other variables controlled, people in states that authorize deputy registrars are no likelier to vote. Authorizing the appointment of deputy registrars in every state would not have an appreciable effect on turnout.

3. *Where to Register.* The location of the registrar's office does not affect the probability that people will vote. Being able to register in one's neighborhood as opposed to going to a municipal or county level office does not increase turnout.

Table 4. Projected Percentage Increase in Turnout by Changing Registration Laws

	Percentage Increase in Turnout if Registration Laws Changed Everywhere to:				
	Elimination of Closing Date	Regular Office Hours	Evening and/or Saturday Registration	Some Absentee Registration	Total Increase[a]
National	6.1	.4	2.5	.5	9.1
North	5.6	.2	2.1	.2	7.8
South	7.3	.9	3.6	1.4	12.8
Whites	5.9	.4	2.4	.5	8.9
Blacks	7.2	.5	3.0	.8	11.3
Northern Whites	5.5	.2	2.1	.2	7.7
Northern Blacks	6.5	.3	2.4	.1	9.1
Southern Whites	7.2	.9	3.5	1.3	12.4
Southern Blacks	8.2	.8	3.9	1.8	14.5
Years of Education					
0–4	8.2	.5	3.3	1.0	13.2
5–7	7.9	.6	3.1	.9	12.6
8	6.9	.4	2.8	.6	10.4
9–11	7.0	.4	2.7	.6	10.4
12	6.1	.4	2.6	.5	9.3
1–3 college	5.4	.3	2.1	.4	7.8
4 college	3.8	.3	1.7	.4	5.6
5 or more college	1.9	.1	.9	.1	2.8
Family Income					
Under $2,000	7.1	.6	2.9	1.0	11.4
$2,000–7,499	6.6	.4	2.8	.6	10.1
$7,500–9,999	6.3	.4	2.6	.5	9.6
$10,000–14,999	5.8	.4	2.4	.4	8.7
$15,000–24,999	5.1	.4	2.1	.3	7.4
$25,000 and over	4.3	.3	1.7	.2	6.2
Age					
18–24	7.2	.4	2.9	.5	11.0
25–31	6.7	.4	2.8	.6	10.2
32–36	6.0	.5	2.5	.5	9.1
37–50	5.5	.4	2.2	.5	8.2
51–69	5.3	.4	2.3	.5	8.1
70–78	6.0	.3	2.1	.5	8.7
79 and over	6.8	.3	2.9	.5	10.4

[a] The number in this column is the projected increase in turnout if each of the provisions at the head of the first four columns were adopted in all states. The first four columns do not add up to the figure in the total column because the joint effect of a combination of changes is not equal to the arithmetic sum of the individual effects.

4. *Years Before Purging.* Periodic purging from the registration rolls of those who did not vote in the previous election does not decrease the likelihood that otherwise eligible citizens will go to the polls. Allowing a citizen who fails to vote to remain on the rolls for eight years does not increase voter turnout above what it would be if he were purged from the rolls for failure to vote in a single election.

Changes in four other areas of registration laws would increase turnout, although in only one respect—the closing date—would the change be substantial.

1. *Absentee Registration.* Ten states did not permit civilian absentee registration. This affects the sick and disabled, and people who are away from home because of business, school, or vacations. Allowing civilian absentee registration for these groups will increase national turnout by about half of one percent. Nearly all this increase would be in the South.

Five states had universal civilian absentee registration, permitting anyone to register by mail, not just the absent, disabled, and sick. This plan received considerable attention in 1976 as Congress debated nationwide postcard registration. The House passed a bill for postcard registration, but only after amending it to delete authority for the government to mail the cards to every household. Thus the burden of obtaining the postcard was left on the individual, although presumably civic and political groups could distribute the cards. The House bill was similar to the system used by five states in 1972. Our estimates indicate that in 1972 this type of registration system did *not* result in higher turnout than the more limited absentee registration discussed in the preceding paragraph. Therefore it looks as if this plan would not have much effect. The crucial amendment to the House bill shifted the initiative from the government to the individual. Our data do not permit us to estimate the impact of a scheme that, like the original bill, removed from the individual the burden of initiating registration proceedings.

2. *Irregular Weekday Office Hours.* Although the location of the registrar's office does not affect the level of voter turnout, the hours it remains open does. Residents of states where registration offices are not regularly open during normal business hours are less likely to vote than are people in states where one can register any time during the business week. Allowing registration at any time during the work week would increase national turnout by about 0.4 percentage points. Turnout in the South would increase by 0.9 percent, compared to 0.2 percentage points in the North.

3. *Evening and Saturday Registration.* People living in states where registration offices are required to remain open outside normal business hours are more likely to vote than are residents of states where offices need not be open in the evening or on Saturday. Requiring evening and/or Saturday registration everywhere in the country would raise turnout by 2.5 percent.

This expansion of office hours would be most beneficial to the less educated. Turnout by people with less than five years of school would increase 3.3 percent, compared to 2.8 percent for grammar school graduates, 2.6 percent for high school graduates, and less than 2 percent for those with a college degree. Evening and/or Saturday registration would have more impact in the South. An additional 3.9 percent of southern blacks and 3.5 percent of southern whites would go to the polls. The comparable figures for the North are 2.4 and 2.1 percent.

4. *Closing Date.* Of the legal changes considered here, the one that would have the greatest impact on the level of voter turnout would be elimination of the closing date. An early closing date decreases the probability of voting. If one could register up until election day itself, when media coverage is widest and interest is greatest, turnout would increase by about 6.1 percentage points. This would have meant an additional eight million voters in 1972. If the closing date were a week before the election everywhere in the country, turnout in 1972 would have been 4.5 percent higher.

Because of the earlier closing dates in the South, people living there would be affected more than northerners by such changes. People with less schooling would benefit most if registration were permitted until election day. Turnout would increase by about 6 percentage points for those without a grammar school education, 4.6 percent for high school graduates, and just 2 percent for people with a college degree.

Our projections indicate that if all the changes summarized in Table 4 had been instituted throughout the country in 1972, turnout in the presidential election that year would have been 9.1 percent higher. This represents 12.2 million additional voters if the registration laws everywhere had been as lenient as in the most permissive states. (With a seven-day closing date, the total increase in turnout would have been 7.7 percent.)

Variations in the Effect of Registration Laws

We have already seen that the effect of relaxing registration laws would not be felt evenly across the population. One reason, of course, is that the gap between the status quo and uniformly permissive provisions is much greater in some places than in others. In 1972 few states came close to the hypothetical permissive situation assumed in Table 4.[41] The laws in the southern states would undergo the biggest change. Therefore making registration easier would have the greatest impact in that part of the country. If all four provisions described in Table 4 were enacted, turnout in the South would increase by approximately 12.8 percentage points while turnout in the North would go up by about 7.8 percent. This difference between the regions is caused by two factors. First, as observed in Table 1, the southern states presently have more restrictive statutes than the rest of the country. Therefore easing the laws would cause greater changes in the South. Second, since those with fewer years of formal education would benefit most from the changes and since the South has a lower mean level of education than the northern states, a greater percentage of southerners would benefit from the changes.

Within the South, the projected increases in turnout for blacks and whites are relatively similar. Whites would vote 12.4 percent more, while black turnout in the South would go up by 14.5 percentage points. In the North, where the gap between the races in educational attainment is smaller, there is less racial difference in projected turnout. An additional 7.7 percent of northern whites would go to the polls, compared to a gain in turnout of 9.1 percent for the blacks.

The most striking variations in the effects of registration reform would be among people at different levels of education.[42] Liberalizing registration provisions would have by far the greatest impact on the least educated, and relatively little effect on well-educated people. Turnout would increase 10.2 percent among people with less than five years of school, by 10.4 percent among elementary school graduates, 9.3 percent among those with a high school diploma, and only 2.8 percent among people with some postgraduate schooling.

Seeing how the costs of voting affect some people more than others aids our understanding of variations in the importance of politics to individual citizens. Before examining this subject, we can dispose of an alternative explanation for the greater impact of restrictive registration laws on the uneducated: that education is in large measure a surrogate for income and occupation. Better-off people, so the argument might go, have more free time for nonessentials like registering and voting, while the harried low-income worker cannot afford long journeys during business hours. But the impact of the laws diminishes with every increment in education. High school graduates and college dropouts are no more able to take time off from work than the less-educated citizens whose behavior is most affected by the laws. A couple of years of college do not admit one to the leisure class. Moreover, data on time use show that blue-collar workers have slightly *more* free time than executives, professionals, and other white-collar people.[43]

It is useful to ponder the costs and benefits of voting, and how they are distributed in the population. The appeal of participating in an election increases with education, and the costs of doing so vary inversely with education. It is little wonder, then, that apparently trivial increments in the burden of registering may raise the cost of voting above the threshold of many people. We see that threshold established by two factors: the individual's level of interest in the election, and his ability to manage the procedural steps required to cast a ballot.

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[41] North Dakota did not require registration at all in 1972. There were not enough respondents from this single, sparsely populated state to permit precise estimates of the effects of its unique voting laws. By 1976 four other states permitted election day registration.

[42] Table 4 also shows the effect of liberalized registration laws by income and age. Poorer people would be most affected, as we would expect because of the relationship between education and income. The variation among income categories is much smaller than among people with different educational attainment, however. This reflects the much stronger relationship between education and turnout. Variations by age are small. People in the age groups with the highest turnout would be affected only slightly less than those with the lowest voting rates—citizens under 32 and over 78.

[43] John P. Robinson and Philip E. Converse, "Social Change in the Use of Time," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage, 1972), pp. 74–75. See our *Who Votes?* for more discussion of the relationship between free time and turnout.

Formal education increases one's capacity for understanding and working with complex, abstract, and intangible subjects (that is, subjects like politics). Acquisition of these skills and facts heightens interest in politics. Schooling also imparts experience with bureaucratic relationships and such simple information-seeking skills as looking up a necessary item in a book. Irrespective of one's degree of political interest, this heightened level of understanding and information would also reduce the costs of registering, even in the most restrictive state. In short, education is likely to increase interest in politics and reduce the costs of manifesting that interest by voting.

The more permissive the registration laws, the lower the time, energy, and information costs of voting. This is of greatest benefit to people whose interest is not sufficient to carry them across the higher threshold imposed by more restrictive provisions. The costs imposed by restrictive laws might be trifling to an educated person, and increasingly daunting to those with little schooling.

Thus for someone who is interested in politics, can anticipate the need for registration before the peak excitement of election eve, and can easily locate the registrar's office, registration is a relatively costless act. On the other hand, for someone whose interest is aroused only a few days before the election, has minimal exposure to information, and is less adept at learning things like places and hours of registration, the whole process is a much more difficult hurdle. The barriers imposed by restrictive laws seem to make little difference to the well-educated, but are a fairly formidable impediment to people with less interest and bureaucratic skill. To put it another way, the difference in turnout produced by variations in registration laws is an indication of the varying commitment and capacity to vote of different kinds of people.

The Political Consequences of Registration Law Reform

Liberalizing the registration laws would expand the electorate and would increase disproportionately the turnout of low-income groups. Because of this differential impact, most scholars, journalists, and politicians have assumed that easier registration would markedly change the social class composition of the electorate. The result would be a less interested and knowledgeable voting population and a windfall of votes for the Democratic party.[44] The prevailing view is expressed in conservative columnist James Kilpatrick's comment on President Carter's election-day registration plan: "The thinly disguised ulterior motive [for Carter's proposal], freely if privately conceded on Capitol Hill, is to benefit the Democratic party. This is a political power play, as brazen as any stunt ever pulled in the bad old days of Tammany Hall. . . ."[45] It seems safe to conclude that these fears help explain Republican and southern Democratic opposition to legislative proposals for making registration easier.

To the extent that such assumptions have an empirical base, it consists of findings about the types of people whose turnout would be most affected by liberalized registration provisions. What is overlooked is the elementary (if not obvious) statistical point that the politically significant facts are not rates of change, but whether the expanded electorate resulting from registration law reform would be politically different from the smaller actual electorate.

Using Equation II we have projected what the demographic, partisan, and ideological composition of the electorate would have been in 1972 if the registration provisions assumed in Table 4 were instituted nationally. We have compared these projections to the composition of the actual electorate, in order to estimate how the electorate would change as a result of making registration easier.[46] The demographic comparisons, based on our original Census sample, are summarized in Table 5. Tables 6 and 7 compare the two electorates with respect to party identification and attitudes on various issues. They are based on data from the 1972 National Election Study of the Center for Political Studies of the University of Michigan.[47] All three tables lead to the same conclusion: the expanded electorate produced by relaxing registration laws would be remarkably similar to the smaller, actual electorate.

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[44] For representative comments, see Nelson W. Polsby and Aaron Wildavsky, *Presidential Elections*, 4th ed. (New York: Scribner, 1976), p. 129. For a dissenting view, emphasizing "the general insensitivity of partisanship to large changes in turnout," see Philip E. Converse, "The Concept of a Normal Vote," in Angus Campbell et al., *Elections and the Political Order* (New York: John Wiley, 1966), p. 29.

[45] *San Francisco Chronicle*, 16 May 1977, p. 38. The Republican National Committee formally opposed Carter's plan, and a Gallup Poll in the spring of 1977 revealed that rank-and-file Republicans were more hostile to it than Democrats, who themselves were unfavorable to the proposal. (*San Francisco Chronicle*, 9 May 1977, p. 8.) The pronounced partisan cleavage on this issue among Congressmen is described in *Congressional Quarterly Weekly Report*, 14 May 1977, pp. 909–11.

[46] The composition of the actual electorate was estimated by using Equation II to compute a probit estimate for each respondent. The probit estimate was then converted to a probability. The percentage of the electorate comprised of people with a given character-

The largest increases would occur in demographic categories. With more permissive registration laws, the electorate would include slightly larger proportions of groups that traditionally vote less. The change, such as it is,

Table 5. Composition of the Actual 1972 Electorate and of the Projected Electorate After Relaxation of Registration Laws

	Northern Electorate		Southern Electorate		Total Electorate	
	Actual	Projected	Actual	Projected	Actual	Projected
Years of Education						
0–4	1.4%	1.6%	3.9%	4.6%	2.0%	2.4%
5–7	3.7	4.0	6.9	7.8	4.4	4.9
8	9.6	9.9	6.1	6.5	8.8	9.1
9–11	14.5	14.9	14.3	15.0	14.5	14.9
12	38.9	39.0	36.5	36.5	38.4	38.3
1–3 college	16.8	16.4	15.2	14.6	16.4	16.0
4 college	8.4	8.0	10.5	9.5	8.9	8.4
5+ college	6.7	6.2	6.5	5.6	6.7	6.1
	100.0%	100.0%	99.9%	100.1%	100.1%	100.1%
Family Income						
Under $2,000	4.2%	4.4%	7.7%	8.1%	5.0%	5.3%
$2,000–7,499	28.9	29.4	35.9	37.0	30.5	31.3
$7,500–9,999	14.5	14.6	13.0	13.2	14.1	14.3
$10,000–14,999	28.7	28.6	23.8	23.2	27.6	27.3
$15,000–24,999	18.0	17.6	15.4	14.5	17.4	16.8
$25,000 and over	5.7	5.5	4.3	3.9	5.4	5.1
	100.0%	100.0%	100.1%	99.9%	100.0%	100.1%
Race						
Whites	92.5%	92.3%	87.2%	86.3%	91.3%	90.8%
Non-whites	7.5	7.7	12.8	13.7	8.7	9.1
	100.0%	100.0%	100.0%	100.0%	100.0%	99.9%
Age						
18–24	14.6%	15.4%	12.5%	13.9%	14.1%	15.0%
25–31	13.9	14.1	15.4	15.7	14.2	14.5
32–36	9.0	8.9	9.5	9.4	9.1	9.1
37–50	25.5	24.9	26.5	25.6	25.7	25.1
51–69	28.1	27.6	27.3	26.6	28.0	27.3
70–78	6.7	6.7	5.7	5.7	6.5	6.4
79 and over	2.3	2.4	3.0	3.1	2.4	2.5
	100.1%	100.0%	99.9%	100.0%	100.0%	99.9%

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istic (e.g. a college education) was computed in the following manner:

$$\frac{\sum_{j=1}^{j} \text{Probability of Voting}}{\sum_{n=1}^{n} \text{Probability of Voting}} \times 100$$

where j = the number of respondents with a given characteristic (i.e. in that category on the variable); and n = the total number of respondents. The same procedure was used to calculate the projected composition of the electorate, with the values for the registration provisions altered to simulate the liberalized conditions.

We used Equation II rather than the reported turnout to estimate the composition of the actual electorate. If we had used the reported turnout, then the difference between the projected electorate and the actual electorate would be due both to the simulated changes in registration laws and the residual for each individual in Equation II. These residuals cancel out if Equation II is used for calculating the composition of both the actual and the projected electorates.

[47] The data were made available by the Inter-University Consortium for Political Research.

would be greatest in the South. In 1972, people who had not graduated from high school comprised 29.7 percent of the nationwide electorate. Relaxing registration provisions would have increased their share of the electorate by 1.6 percent. In the North, this category would have accounted for an additional 1.2 percent of the voting population. In the South, the increase would be 2.7 percent. *This latter figure is the largest shift in the composition of the electorate that we could find in any descriptive category.* College-educated voters would account for a correspondingly smaller share of the electorate, down from 31.9 to 30.6 percent in the North, from 32.2 to 29.7 percent in the South, and 32 to 30.5 percent in the country as a whole.

Differences in income are generally more important to political choice than are educational cleavages, and here the impact of registration reform would be even smaller. The proportion of the national electorate comprised of people with annual family incomes of less than $10,000 would increase by 1.3 percent. The gain would be 0.8 percent in the North and 1.4 percent in the South.

Projected changes in the racial composition of the electorate would be smaller still. Nonwhites would comprise an additional 0.3 percent of the national electorate. The increase would be 0.2 percent in the North and 0.9 percent in the South.

Finally, the proportion of the national electorate comprised of people under the age of 32 would increase from 28.3 percent to 29.5 percent. The increase would be 1.1 percent in the North and 1.7 percent in the South.

In sum, making it easier for people to register, although it would increase turnout, would have a very small impact on the demographic composition of the electorate. Voters in the aggregate would be marginally less educated, poorer, blacker, and younger. These changes would be somewhat greater in the South.

Some people have speculated that, with a bigger share of the electorate young, poor, and uneducated, voters would be substantially less interested in politics.[48] As one might suspect on the basis of the demographic projections just summarized, changes in this regard would be very modest.[49] Liberalizing registration laws would increase by 0.6 percent the proportion of the electorate who have hardly any interest in the campaign. An additional 0.8 percent of the electorate would be comprised of those who follow politics only now and then or hardly at all. The electorate's level of attentiveness to politics would not be significantly altered by relaxing voter registration laws.

These findings about the minimal changes in the demographic composition of the electorate are inconsistent with the general belief that easier registration laws would be a major boon for Democrats. Our projection, in Table 6, confirms doubts about the validity of the conventional wisdom. The partisan composition of the hypothetical expanded electorate would be virtually identical to that of the actual electorate. In 1972, 36.4 percent of those voting were aligned with the Republican party, as outright identifiers or Independents who said they were "closer to" the Republicans.[50] With more liberal registration laws, Republicans would comprise 35.9 percent of the electorate—a wholly insignificant shift. The Democratic windfall would be equally trivial—a gain of 0.3 percent. The comparative popularity of McGovern and Nixon would be identical in the actual and hypothetical electorates.[51]

Although the partisan balance of the electorate would not be affected by registration reform, is it possible that these changes would increase the voting strength of people with particular attitude configurations? It has been suggested, for example, that this would result in more ballot strength for people who are liberal on economic issues but conservative on social and racial issues. Comparing the actual and hypothetical electorates on a number of issue attitudes measured in the 1972 Michigan study, we found traces of this pattern, but the differences were inconsistent and invariably tiny. Some comparisons of attitude distributions in the actual and projected electorates are presented in Table 7. This table reports the *largest* differences we found on issues.

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[48] See Polsby and Wildavsky, *Presidential Elections*, pp. 239–41; and V. O. Key, *Politics, Parties, and Pressure Groups*, 5th ed. (New York: Thomas Y. Crowell, 1964), p. 590.

[49] These projections, as well as those summarized in Tables 6 and 7, are based on the 1972 National Election Study of the University of Michigan Center for Political Studies. For this sample we used Equation II to estimate both the actual and projected composition of the electorate, as described in note 46. We deleted 13 cases from this sample: the 10 respondents from the District of Columbia and 3 respondents whose education was not reported, and for whom turnout therefore could not be predicted. Equation II correctly predicted turnout for 74.3 percent of the cases in this sample, compared to 71.4 percent of the cases in the Current Population Survey subsample. The differences between the actual and projected electorates summarized in Table 5 can be replicated within 0.4 percentage points by using Equation II with the Michigan data.

[50] This classification is based on Bruce E. Keith, et al., "The Myth of the Independent Voter" (paper delivered at the 1977 Annual Meeting of the American Political Science Association).

[51] Candidate popularity was measured by the proportion of those rating McGovern higher than Nixon on the candidate "feeling thermometers."

With the electorate expanded through permissive registration, the proportion of voters who say that the federal government should see that everyone has a job and an adequate standard of living would increase by 0.3 percentage points. Support for government-sponsored health insurance would increase by 0.1 percent. The effect of registration reform on voters' support for social welfare issues, in short, would be negligible.

Virtually the same can be said of other issue domains. The expanded electorate would include 0.7 percent more people who think the federal government should shun involvement in school integration, and 0.3 percent more opponents of school busing. The picture is the same on "social issues." Support for the pro-

Table 6. Partisan Composition of the Actual and Projected Electorates in 1972

Party Identification	Actual Electorate	Projected Electorate
Strong Democrat	14.6%	14.7%
Weak Democrat	25.3	25.5
Independent Democrat	11.2	11.2
Independent	12.4	12.6
Independent Republican	10.9	10.9
Weak Republican	14.2	14.0
Strong Republican	11.3	11.0
	99.9%	100.0%
(N)	(1,809)	(2,044)

Table 7. Issue Attitudes of the Actual and Projected Electorates in 1972

	Actual Electorate	Projected Electorate
Government should see everyone has a job and good standard of living	30.4%	30.7%
Middle of the road	23.9	23.7
People should get ahead on own	45.7	45.6
	100.0	100.0
Federal government should ensure school integration	44.7%	44.0%
Government should stay out of this issue	55.3	56.0
	100.0	100.0
Legalize marijuana	22.7%	22.3%
Middle of the road	11.3	11.1
Increase penalties for use	66.0	66.6
	100.0	100.0
Women deserve equal role in business and government	50.0%	49.7%
Middle of the road	20.8	20.7
Women's place is in the home	29.2	29.6
	100.0	100.0
Self-described liberals	25.8%	25.8%
Moderates	36.2	36.5
Conservatives	38.0	37.7
	100.0	100.0

position that woman's place is in the home would increase by half a percentage point. Opponents of legalized marijuana would make up an additional 0.6 percent. The proportion of voters who identify themselves as liberals would remain unchanged. In short, the ideological composition of the expanded electorate would be virtually identical to that of the actual electorate in 1972.

Conclusions

We have found that if every state had had registration laws in 1972 as permissive as those in the most permissive states, turnout would have been about nine percentage points higher in the presidential election. Registration provisions have most effect on the least educated.

The political consequences of registration law reform are measured by comparing the actual electorate to the hypothetical electorate estimated by projecting rates of change in turnout. This comparison shows that relaxing registration requirements would produce a voting population very similar to the actual 1972 electorate. The electorate would be expanded with virtually no change in its demographic, partisan, or ideological characteristics. It would be bigger, but not different.

How should we assess the significance of our findings about the effect of laws on turnout? Is a nine percent increase in voter turnout disappointingly small, or gratifyingly substantial? Measured against the rhetoric of some champions of voter registration reform, nine percent is fairly small potatoes. But political rhetoric is seldom a good yardstick for policy analysis.

The changes we have considered are within a narrow compass of alternatives. One question that remains is how much national voter turnout would increase if the burden for registration shifted from the individual to the government. What would be the increase in turnout if, as in most democracies, the government took the initiative for registering voters by establishing national voter lists through a door-to-door canvass or by mailing postcards to all citizens? Our data do not allow us to make a precise estimate of the impact of a registration system in which the government bore responsibility for establishing voter eligibility and where virtually all the costs of registering were removed from the individual. Given the results that would be achieved by the relatively minor changes discussed above, we are confident that establishing a European-type registration system would increase voter turnout by substantially more than nine percent.

We should note that the registration laws are not the only environmental variables affecting turnout. The modern peak of voter turnout was reached in 1960, when one- and two-year residency requirements, poll taxes, and literacy tests were common; and when millions of southern blacks were disenfranchised through maladministration of the laws. Since then all these barriers have been removed, the nation's educational level has risen substantially, and turnout has fallen. The 18-year-old vote does not explain the drop in turnout. Other aspects of the political environment clearly are at work. This caution, however, should not distract us from the finding of this paper that registration laws have a substantial effect on the number of people who go to the polls on election day.

Appendix A
Samples for the Study of Turnout

Estimates of turnout in sample surveys are always somewhat higher than those based on the total number of ballots cast by the aggregate voting-age population. Since 1948, reported turnout in sample surveys has ranged between 5 percent and 17 percent higher than the aggregate estimates.[a] The 1972 election is no exception, as Table A demonstrates. The commonly cited aggregate turnout figure is 55.4 percent, while reported turnout from our Current Population Survey sample is 66.7 percent, a gap of 11.3 percent. The turnout reported by the Michigan study is 72.8 percent, 17.4 percent above the aggregate estimate.

Observers of this persistent gap between survey reports and "the real figure" sometimes jump to the conclusion that the discrepancy reflects only misreporting by respondents reluctant to admit that they did not do their civic duty. This is only one of a number of contributing factors, however. First, it should be understood that the aggregate percentage *underestimates* turnout. The denominator of the percentage is an estimate of the total number of people of voting age.[b] This includes millions of people who are ineligible to vote: aliens, inmates of prisons and mental institutions, and ex-convicts, who cannot vote in many states.[c] If noncitizens are removed from the denominator of the aggregate computation, the turnout estimate rises from 55.4 to 56.7 percent. The numerator of the aggregate percentage represents the total number of valid counted votes cast for president. This excludes people who cast a spoiled ballot (they think they voted, but their vote is not counted), those who go to the polls but do not vote for president, and people whose votes for miscellaneous write-in candidates are not tallied. The best available estimates indicate that 2 to 3 million people are "uncounted voters."[d]

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[a] For extended discussions of this problem see Aage R. Clausen, "Response Validity: Vote Report," *Public Opinion Quarterly*, 32 (Winter 1968–69), 588–606; William G. Andrews, "American Voting Participation," *Western Political Quarterly*, 19 (December 1966), 639–52; Elmo Roper, "How to Lose Your Vote," *Saturday Review*, 18 March 1961, pp. 14–15; "Voting and Registration in the Election of November 1972," pp. 7–8; and *The American Voter*, pp. 94–96.

[b] Bureau of the Census, "Projections of the Population of Voting Age for States: November 1972," *Current Population Reports*, March 1972, Series P–25, No. 479, pp. 1, 3.

Survey estimates of turnout are also inflated by sample design. Most surveys do not incude some categories of people who are eligible to vote but rather unlikely to do so. Students in dormitories, people living on military reservations in the United States, and inmates of institutions for the elderly are all left out of the Census sample. The institutional population amounted to 5.8 million people in 1970. In addition to such deliberate exclusions, surveys consistently tend to undersample some demographic groups with low turnout rates, and to oversample others which are more likely to vote.[e]

Finally, there is the problem of response error, generally in the form of respondents falsely claiming that a vote was cast. One estimate of overreporting, based on a comparison of 1964 survey responses with the voting records, suggested that response error was about 8 percent.[f] Clearly, some response error exists in our data, although we cannot say just how much. We feel safe assuming that the response error is uncorrelated with the registration statutes. On the basis of the foregoing discussion, however, we caution readers against assuming that overreporting is the sole cause of the discrepancy between our estimated turnout and the aggregate figure.

The additional discrepancy of 6.1 percent between reported turnout in the Census survey and the Michigan Center for Political Studies survey can be attributed to three factors.[g] First, the Census Bureau had a higher interview completion rate. In 1972 the Census completed interviews with 93.7 percent of those selected for its sample, compared to 84.4 percent for the post-election interview in the Michigan study.[h] Respondents whose post-election inter-

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[c] Ibid., p. 3; Clausen, "Response Validity: Vote Report," pp. 589–91; Meyer Zitter and Donald E. Starsinic, "Estimates of 'Eligible' Voters in Small Areas: Some First Approximations," *Proceedings of the Social Statistics Section of the American Statistical Association*, 1966, pp. 368–78; and Andrews, "American Voting Participation," pp. 642–44.

[d] *The American Voter*, pp. 94–95; and "Registration and Voting in the Election of November 1972," p. 8.

[e] Clausen, "Response Validity: Vote Report," pp. 592–96; "Voting and Registration in the Election of November 1972," pp. 8, 13; and *The American Voter*, pp. 94–95.

[f] Clausen, "Response Validity: Vote Report," pp. 596–98, 601. For other estimates see *The American Voter*, pp. 93–96; and "Voting and Registration in the Election of November 1972," p. 7.

[g] These were drawn from Clausen, "Response Validity: Vote Report," pp. 594–95.

[h] Center for Political Studies, *1972 Study*, p. xiii; and Bureau of the Census, "Voting and Registration in the Election of November 1974," *Current Population Reports*, Series P–20, No. 293, p. 89.

Table A. Estimates of Voter Turnout in the 1972 General Election

	Estimated Voter Turnout
Proportion of voting age population casting votes for president[a]	55.4%
Proportion of citizens of voting age casting votes for president[b]	56.7
Current Population Survey[c]	66.7
Center for Political Studies National Election Study[d]	72.8

[a] Bureau of the Census, "Projection of the Population of Voting Age for States: November 1974," *Current Population Reports*, Series P–25, No. 526, September 1974, p. 8, Table 3. The number of votes cast is based upon: U.S. Congress, Clerk of the House, *Statistics of the Presidential and Congressional Election, 1972.*

[b] Letter to authors from Gilbert R. Felton, Population Division of the Bureau of the Census, 29 December 1976.

[c] *Current Population Reports*, Series P–20, No. 253, our analysis. (We excluded from the sample noncitizens and cases where the respondent did not know if a vote had been cast. This produced a weighted N of 128,582 cases.)

[d] Variable 0477 (N = 2,283).

views could not be completed tended to come from groups with lower turnout rates.[i] Second, the sample designs differ. The Census sample includes the "rooming-house population," a low-turnout group excluded from the Michigan sample. Third, the Census has a few political questions only in the November interview, compared to the heavily political content of the pre- and post-election interviews conducted by the Michigan study. This raises the possibility that some Michigan respondents were politically stimulated by the pre-election interview, a possibility that is precluded in the Census study.[j] Needless to say, these three points of difference suggest further advantages of the Census survey for estimating voter turnout.

Appendix B
Description of the Variables and Coding Procedures

Individual level data. All individual level data were taken from the November 1972 Bureau of the Census Current Population Survey described above.

TURNOUT: Did not vote = 0; Voted = 1.

REGION: North = 0; South = 1 (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas and Virginia).

RACE: Whites = 0; Non-whites = 1.

EDUCATION: 0–4 years = 1; 5–7 years = 2; 8 years = 3; 9–11 years = 4; 12 years = 5, 1–3 years of college = 6; 4 years of college = 7; 5+ years of college = 8.

AGE: The number coded was the actual age of the respondent. (People over the age of 99 were coded "99" by the Bureau of the Census.)

Registration Provisions.

RESIDENCY REQUIREMENT: The coding was done so that the variable is the *de facto* residency requirement in the state. By this we mean the following: If a state has no residency requirement but the last day new residents can register is 30 days before the election, then the state has a 30-day *de facto* residency requirement. Even though there is no explicit residency requirement, new residents must have been in the state at least 30 days prior to the election in order to register. This state is coded as having a 30-day *de facto* residency requirement, not a zero day requirement. Similarly a state that had a closing date of 30 days but a special provision for new residents to register up to 7 days before the election, was coded as having a 7-day *de facto* residency requirement.

CLOSING DATE: The number coded was the number of days before the election on which a resident could last register to vote.

WHERE TO REGISTER: County office only = 1; Other = 0. Precinct or neighborhood locations below the town level = 1; Other = 0.

DEPUTY REGISTRARS: Allowed = 0; Not allowed = 1.

MONDAY THROUGH FRIDAY REGISTRATION OFFICE HOURS: Regular Hours (40 hours a week, every week) = 0; Irregular Hours = 1.

EVENING AND/OR SATURDAY REGISTRATION HOURS: Not required = 0; Regularly required = 1.

ABSENTEE REGISTRATION BY MAIL: Permitted for ill, disabled and/or absent from voting unit = 0; None permitted = 1. Universal absentee registration = 1; Other = 0.

YEARS BEFORE PURGING: The number of years of non-voting after which one's name is removed from the registration rolls. Number of years coded = 2–8; No purging = 8. (Our preliminary estimates were made using two variables. The first was coded 2–8 years, with no purging coded 0. A separate dummy variable was included (1 = no purging) to estimate this category. There is no significant loss of information by using the single variable described above.

Other Variables

HOURS POLLS OPEN: The number coded was the number of hours the polls are open on election day.

GUBERNATORIAL ELECTION: No concurrent gubernatorial election = 0; Gubernatorial election = 1.

SENATORIAL ELECTION: Concurrent senatorial election = 0; No senatorial election = 1.

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[i] *The American Voter,* p. 95; and Clausen, "Response Validity: Vote Report," pp. 592–93.

[j] Cf. R. E. Kraut and J. B. McConahay, "How Being Interviewed Affects Voting: An Experiment," *Public Opinion Quarterly,* 37 (Fall 1973), 398–406; and Richard F. Yalch, "Pre-Election Interview Effects on Voter Turnout," ibid., 40 (Fall 1976), 331–36.

Appendix C
Probit Analysis

In the probit model the conditional probability of voting $p(x)$ is defined as

$$p(x) = F(\beta_0 + \beta_1 + \ldots + \beta_n X_n)$$

or, equivalently,

$$F^{-1}(p(x)) = \beta_0 + \beta_1 X_1 + \ldots + \beta_n X_n$$

where $F(\ )$ represents the value of the cumulative standard normal distribution. Using numerical maximum methods (because the normal equations are non-linear in the parameters) the parameters $\beta_0, \beta_1 \ldots, \beta_n$ can be estimated.

Figure 1 illustrates the differences between the OLS model and the probit model. The OLS model assumes a linear relationship between the parameters and the probability of voting; a unit increase in an independent variable has the same impact on the probability of voting regardless of the values of the other independent variables. For example, consider an OLS equation in which a registration requirement (represented by a dummy variable) has a coefficient of –.13. We would conclude that living under this requirement decreases the probability of voting by 13 percent (–.13 x 1 x 100%). That is, a person who is otherwise 90 percent likely to vote would have a probability of voting of 77 percent as a result of this statute. The effect would be the same for everyone: a probability of voting of 60 percent would be reduced to 47 percent; a 40 percent probability would now be 27 percent; and a person who is otherwise 10 percent likely to vote would have a probability of voting of –.03!

As we have argued, there is reason to believe that the impact of registration statutes on the probability of voting is not constant across all individuals. Rather, the effect varies with the probability that the individual would otherwise vote. A college graduate who is 90 percent likely to vote is much less affected by a registration statute than a high school dropout who is only 55 percent likely to vote.

The probit model takes this into account. Returning to our previous example, suppose the registration requirement had an estimated probit coefficient of –.13. Among those people who are either 10 percent likely to vote or 90 percent likely to vote, the probability of voting would decrease by approximately 2 percent. Among those who are between 40 and 60 percent likely to vote, the probability of voting would decrease by approximately 5 percent. The variable has the smallest impact on those who are either least likely to vote or nearly certain to vote. It has the greatest impact in the middle of the distribution, on those who are between 40 and 60 percent likely to vote.

As one can see, a probit equation is not as easy to interpret as the familiar OLS equation. Unlike OLS, the probit coefficient does not directly represent an estimate of the amount of change in the dependent variable resulting from a unit increase in the independent variable. Rather, with probit analysis, the coefficient is an estimate of the amount of change on the cumulative standard normal distribution that would result from a change in one unit in the independent variable, with the other variables held constant. This value is then easily converted to a probability.

There are ways to compensate for the linearity of the coefficients in the OLS model. The most common approach is the inclusion of interaction terms which allow the effect of one independent variable to vary with the value of another independent variable. However, two problems still remain.

As suggested by our example and by Figure 1, with an OLS equation the predicted probability of voting will not necessarily be confined to the 0–1 interval. That is, with OLS

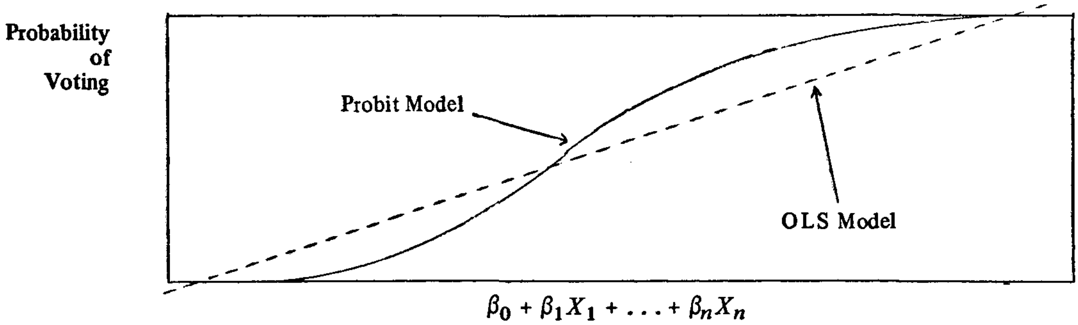

Figure 1.

some values of the independent variables may yield estimated probabilities that are less than 0 or greater than 1. The probit estimates are always between 0 and 1.

Second, with OLS the error variance is not constant across all observations. The variance of the residuals will be small for predicted probabilities close to either 0 or 1 while the variance will be much larger for predictions between .4 and .6. As a result, the OLS estimates are inefficient. See D. J. Finney, *Probit Analysis,* 3rd ed. (Cambridge: Cambridge University Press, 1971); Arthur S. Goldberger, *Econometric Theory* (New York: John Wiley, 1964), pp. 248–51; D. R. Cox, *The Analysis of Binary Data* (London: Methuen, 1970), pp. 1–29; Eric A. Hanushek and John E. Jackson, *Statistical Methods for Social Scientists* (New York: Academic Press, 1977), Chapter 7; and John Aldrich and Charles F. Cnudde, "Probing the Bounds of Conventional Wisdom: A Comparison of Regression, Probit, and Discriminant Analysis," *American Journal of Political Science,* 19 (August 1975), 571–608.

Appendix D
Equation I—Preliminary Estimates of the Effect of Demographic Variables and Registration Laws on Turnout in 1972

Variable	Probit Estimate	Standard Error
(Constant)	–2.7646	.2740
Education	.1838	.0451
Education squared	.0121	.0050
Age	.0706	.0045
Age squared	–.0006	.0001
Race	–.0189	.0549
Region	–.1279	.0546
Residency requirement	.0025	.0018
Closing date	–.0089	.0021
No deputy registrars	.0161	.0448
Irregular office hours	–.1540	.0547
Open evening and/or Saturday	.01271	.0398
Years before purging	.0098	.0105
Register only at county seat	–.0165	.0494
Register in neighborhood	.0564	.0484
No absentee registration	–.1054	.0421
Universal absentee registration	–.0891	.0627
Hours polls open	.0357	.0175
Gubernatorial election	.0441	.0369
No Senatorial election	–.0293	.0385

Log of the likelihood function = –4442.82
–2 times the log likelihood ratio = 1160.27
Degrees of freedom = 19
Number of cases = 7936

Policy Voting in Britain: The Colored Immigration Issue in the 1964, 1966, and 1970 General Elections*

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Several studies in recent years have examined the question of mass issue voting in the United States and have found that people vote more frequently on the basis of policy questions than has heretofore been thought. It would seem to be useful for the study of comparative politics to explore mass policy voting in other democratic countries. The colored immigration issue in Britain is a particularly appropriate one to examine because of the controversy surrounding its impact on voting behavior, especially in the 1970 election. Are the policy preferences of the electorate related to their voting behavior? This question is examined longitudinally through a secondary analysis of the Butler-Stokes election surveys of 1964, 1966, and 1970 for England.

After utilizing controls for a large number of variables, one finds that the immigration issue had no significant impact on electoral behavior in 1964 and 1966. In the 1970 election, however, the Conservatives gained an estimated increment of 6.7 percent in votes because many people perceived them to be the party more likely to keep immigrants out and voted in accordance with that perception. This impact can be attributed to Enoch Powell's associating the Conservative party with restrictive immigration control in the public mind.

Recent studies examining the question of mass issue voting in the United States have found that people vote more frequently on the basis of policy questions than has heretofore been thought.[1] These findings have clashed with the conventional wisdom of American election studies, which maintains that issues are of little importance in determining electoral outcomes.[2] Contemporary scholarship has pointed out that while most voters do not consider a whole set of issues when making up their minds for whom to vote, many do have some issue or issues in mind. Individuals do attempt to make their electoral choices consonant with their positions on the issues most important to them. It has also been contended that the importance of issues as a source of impact upon the voters' decisions varies from election to election. Issues have been found to

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[1] V. O. Key, *The Responsible Electorate* (Cambridge, Mass.: Belknap Press, 1966); Richard W. Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June 1972), 429–49; Martin Fishbein and Fred S. Coombs, "Basis for Decision: An Attitudinal Approach Toward an Understanding of Voting Behavior" (paper delivered at Annual Meeting of the American Political Science Association, Chicago, 1971); Gerald M. Pomper, "From Confusion to Clarity: Issues and American Voters, 1956–1968," *American Political Science Review*, 66 (June 1972), 415–28; Mark A. Schulman and Gerald M. Pomper, "Variability in Electoral Behavior: Longitudinal Perspectives from Causal Modeling," *American Journal of Political Science*, 19 (February 1975), 1–18; John E. Jackson, "Issues, Party Choices, and Presidential Votes," *American Journal of Political Science*, 19 (May 1975), 161–85; Arthur H. Miller et al., "A Majority Party in Disarray: Policy Polarization in the 1972 Election," *American Political Science Review*, 70 (September 1976), 753–78.

[2] Paul F. Lazarsfeld, Bernard R. Berelson, and Hazel Gaudet, *The People's Choice*, 3rd ed. (New York: Columbia University Press, 1968); Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954); Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston, Illinois: Row, Peterson, 1954); Angus Campbell et al., *The American Voter* (New York: John Wiley, 1960); Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David E. Apter (Glencoe, Ill.: Free Press, 1964), pp. 206–61; and Angus Campbell et al., *Elections and the Political Order* (New York: John Wiley, 1966). A recent critique of the issue-voting findings is Michael Margolis, "From Confusion to Confusion: Issues and the American Voter (1956–1972)," *American Political Science Review*, 71 (March 1977), 31–43.

be more significant in the presidential elections of the 1960s and 1970s than in previous elections.

Few studies, however, have examined whether mass policy voting exists in countries other than the United States. Britain is a particularly appropriate setting for such an examination because of its "responsible parties," which supposedly translate mass preferences into governmental policy through the mechanism of presenting contending positions to the voters for their endorsement.[3] In a system featuring responsible parties, an election is thought to be the clearest expression of mass preferences on policy issues. This article will explore whether mass opinion on one controversial issue, the entrance of nonwhite immigrants (termed "colored" immigrants by the British) into the United Kingdom has been reflected in electoral behavior during recent British general elections.

An Overview of Previous Research

The basic thrust of electoral studies in Britain has been to cast doubt on the role of issues in influencing how people vote. The alleged issue-consciousness of the British voter under a system of responsible parties has not been upheld empirically. This was especially true in the 1950s and early 1960s when students of voting behavior could find little relationship between opinions on issues and party choice.[4] Traditional party attachments, the class image of the parties, and perceptions of the leaders' competence seemed to have much more influence on voter choices than did policy issues.

No study of British electoral behavior argued strongly for the importance of issues in determining voting choice until Butler and Stokes found that opinions on economic and social welfare issues had an appreciable relationship to individual electoral choices in the general elections of 1964 and 1966. The economy continued to be a salient voting issue in 1970.[5] Even Butler and Stokes, however, did not specify how important these policy issues were in comparison to the multitude of other variables which influence electoral behavior.

Previous studies of the electoral impact of the colored immigration issue have drawn various conclusions. There is general agreement that the immigration issue affected voting behavior in a small number of West Midlands constituencies in 1964, to the benefit of the Conservative party.[6] These were, however, exceptional cases, and no argument has ever been put forward that the immigration issue had national impact in 1964.

The published findings indicate that immigration had even less impact in the 1966 election. The few seats where immigration was thought to have affected votes in 1964 swung back toward Labour in 1966. Since no other seats seemed to have been affected by opinions on the immigration question, the remark of a victorious Labour candidate that "we have buried the immigration issue" was appropriate at the time.[7] While Butler and Stokes agreed with the general conclusion that opinions on the colored immigration issue had very little impact on the 1964 and 1966 general elections, their analysis found immigration to be an issue of "high potential." That potential was not realized in 1964 and 1966 because the voters failed to differentiate between the positions of the two major parties on the issue.[8]

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[3] The latest statement on the responsible parties controversy, with copious references to previous writings on the subject, is Evron M. Kirkpatrick, "'Toward a More Responsible Two-Party System': Political Science, Policy Science, or Pseudo-Science?" *American Political Science Review*, 65 (December 1971), pp. 965–90.

[4] See Mark Benney, A. P. Gray, and R. H. Pear, *How People Vote* (London: Routledge and Kegan Paul, 1956), pp. 140–46; R. S. Milne and H. C. Mackenzie, *Marginal Seat, 1955* (London: Hansard Society, 1958), pp. 117–21, 126; Mark Abrams and Richard Rose, *Must Labour Lose?* (Harmondsworth, Middlesex: Penguin Books, 1960), pp. 19–20; Jean Blondel, *Voters, Parties, and Leaders*, rev. ed. (Baltimore: Penguin Books, 1966), pp. 77–87; Peter G. J. Pulzer, *Political Representation and Elections* (New York: Praeger, 1967), pp. 114–15; and R. J. Benewick et al., "The Floating Voter and the Liberal View of Representation," *Political Studies*, 27 (June 1969), 191–92, 194.

[5] David Butler and Donald Stokes, *Political Change in Britain*, 2nd ed. (New York: St. Martin's Press, 1974), pp. 296–302; 382–83, 414–16.

[6] Paul Foot, *Immigration and Race in British Politics* (Harmondsworth, Middlesex: Penguin Books, 1965); Nicholas Deakin, ed., *Colour and the British Electorate, 1964* (London: Pall Mall Press, 1965); David Butler and Anthony King, *The British General Election of 1964* (New York: St. Martin's Press, 1965), pp. 224–25, 296; and Butler and Stokes, *Political Change in Britain*, pp. 304–05.

[7] N. D. Deakin et al., "Colour and the 1966 General Election," *Race*, 8 (July 1966), 17–42; David Butler and Anthony King, *The British General Election of 1966* (London: Macmillan, 1966), pp. 250–53, 268–69, 289; Nicholas Deakin, "British Voters and the Immigration Issue," Appendix VII in Sheila Patterson, *Immigration and Race Relations in Britain, 1960–67* (London: Oxford University Press, 1969), pp. 417–22.

The role of the immigration issue in influencing the outcome of the 1970 election has been a subject of considerable controversy. Several analyses have contended that opinions on immigration policy did not have the actual impact that the preelection attention to the issue might have indicated,[9] while other analyses have argued that the immigration question did substantially affect voting behavior in 1970.[10] Nevertheless, while favoring the latter position, even Butler and Stokes admit that the evidence for their conclusions is tenuous at best.[11] That the immigration issue was an important factor in determining the outcome of the 1970 general election has yet to be demonstrated convincingly.[12]

Data and Methods

This study relies most heavily on a secondary analysis of the postelection surveys of Butler and Stokes in 1964, 1966, and 1970. The number of people interviewed after each election ranged from 1200 to 2000, a suitably large sample for multivariate analysis.[13] This body of data constitutes the largest and richest source of information on the individual attributes and political attitudes of the British electorate yet assembled. Among the individual socioeconomic attribute variables used in this analysis are age, social class, subjective social class, income level, years of education completed, kind of school attended, religious affiliation, and pattern of church attendance.

In addition, variables taken from the ten percent sample census of Britain in 1966 are employed as controls. The aggregate data of the census are assigned to individuals according to their constituencies of residence. Social context variables utilized from this data set include region of residence, percentage of New Commonwealth immigrants in the constituency, cars per private household, percentage of males engaged in agricultural occupations, and percentage of males engaged in professional occupations. Also used are the stands of Conservative and Labour parliamentary candidates on the immigration issue, as ascertained by examination of their published election addresses.[14]

The pattern of concentration of colored immigrants within the United Kingdom is responsible for the focus being upon the behavior of English voters only, rather than upon those of Britain as a whole. The degree of concentration of immigrants has been found to be an important factor in the white reaction to the immigration issue.[15] Of the four countries of the United Kingdom, colored immigration has been a substantial phenomenon only in England. There are no recorded colored immigrants in Northern Ireland; Scotland and Wales contain very few. According to the 1966 Census, New Commonwealth immigrants made up 1.8 percent of the total population of England and Wales. In Wales alone, however, New Commonwealth immigrants accounted for only 0.3 percent of the population, in contrast to 2.0 percent in England. In Scotland immigrants were 0.5 percent. Furthermore, of the 132 constituencies with 2.0 percent or more New Commonwealth immigrants in 1966, 130 were in England.[16] By focusing upon the reaction of English voters to the colored immigration issue, one can limit the investigation to the portion of the United Kingdom most affected by colored immigration, yet with considerable variation in the concentration of immigrants.

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[8] Butler and Stokes, *Political Change in Britain*, pp. 303–05.

[9] Nicholas Deakin and Jenny Bourne, "Powell, the Minorities, and the 1970 Election," *Political Quarterly*, 41 (October-December 1970), 399–415; David Butler and Michael Pinto-Duschinsky, *The British General Election of 1970* (London: Macmillan, 1970), pp. 341, 405–07; Gerald Hoinville and Roger Jowell, "What Happened in the Election?" *New Society*, 16 (2 July 1970), 14; Ivor Crewe and Clive Payne, "Another Game with Nature: An Ecological Regression Model of the British Two-Party Vote Ratio in 1970," *British Journal of Political Science*, 6 (January 1976), 68–73.

[10] See Richard Crossman, "Understanding the Profusion of Shrinking Violets," *The Times*, 6 September 1972; John Wood, ed., *Powell and the 1970 Election* (Kingswood, Surrey: Elliot Right Way Books, 1970); and Butler and Stokes, *Political Change in Britain*, pp. 305–08, 415. The post-election analyses offered by Louis Harris Research and National Opinion Polls in *The Polls and the 1970 Election*, ed. Richard Rose (Glasgow: University of Strathclyde Survey Research Centre Occasional Paper No. 7, 1970), pp. 24–25, 39–40, indicate that the combination of the immigration issue and Powell's personality significantly influenced voting behavior. A similar view is expressed in R. W. Johnson and Douglas Schoen, "The 'Powell Effect': or How One Man Can Win," *New Society*, 37 (22 July 1976), 168–72.

[11] Butler and Stokes, *Political Change in Britain*, pp. 306–08.

[12] Preliminary analyses of the two British general elections in 1974 indicate that immigration was not an important factor in the outcome. See A. D. Fox, "Attitudes to Immigration: A Comparison of Data from the 1970 and 1974 General Election Surveys," *New Community*, 4 (Summer 1975), 167; David Butler and Dennis Kavanagh, *The British General Election of February, 1974* (New York: St. Martin's Press, 1975), pp. 24, 332; Community Relations Commission, Reference and Technical Services Division, *Participation of Ethnic Minorities in the October General Election, 1974* (London: Community Relations Commission, 1975), pp. 33–43.

[13] Because of the large number of variables employed and the fact that a few questions were asked of only half-samples, the working N for much of the analysis is considerably smaller than this. A check of the major variables of interest in this analysis did not indicate any significant bias in the results because of this.

[14] Details on the measurement and selection of all background variables—individual socioeconomic, social context, and political context—are available in Donley T. Studlar, "The Impact of the Colored Immigration Issue on British Electoral Politics, 1964–1970" (Ph.D. dissertation, Indiana University, 1975).

[15] David Butler and Donald Stokes, *Political Change in Britain*, (New York: St. Martin's Press, 1969), pp. 351–53; Deakin, "British Voters and the Immigration Issue," pp. 418–19; Christopher Bagley, *Social Structure and Prejudice in Five English Boroughs* (London: Institute of Race Relations,

Since the study of issue voting involves an assessment of how much a policy issue helps or hurts a major political party in an election, we will consider only votes for the two major parties in England.[17] Those who abstained from voting (found to be almost equally distributed across the four categories of opinion on immigration) are also excluded from the analysis. In the absence of any strong theoretical reason for including them in the analysis, it simplifies matters empirically to eliminate abstainers from consideration.[18]

The major techniques employed to analyze the data are simple crosstabulation, correlation, and path analysis through the use of multiple regression. Discoveries in the statistics of social science have allowed the incorporation of nominal and ordinal data into the regression model through the use of dummy variables and the treatment of ordinal variables as if they were interval.[19] These methods, only now gaining widespread acceptance, are particularly appropriate for this study because of the combination of nominal, ordinal, and interval level data involved. By fitting nominal and ordinal data into the regression model, one can assess the comparative importance of the various categories of variables.[20]

1970), p. x; Deakin, *Colour and the British Electorate, 1964*, pp. 163–66.

[16] Great Britain, General Register Office, *Census 1966: United Kingdom General and Parliamentary Constituency Tables* (London: Her Majesty's Stationery Office, 1969), pp. 79–88.

[17] Attempting to analyze votes for other parties, even the Liberals, presents a problem because of the low N in those categories and the multiple controls employed in this analysis.

[18] Bivariate correlation tests for the effect of opinion were run with the abstainers and Labour voters together in one category of the dependent variable and the Conservatives in the other. The results did not differ appreciably from the same analysis with the abstainers removed. When one compares the two-party distribution of the vote in the Butler-Stokes surveys with the actual recorded distributions for the two-party vote in England for the 1964, 1966, and 1970 election, he finds that the respondents in the Butler-Stokes surveys tend toward Labour in reported voting behavior slightly more than the population at large. This discrepancy is not large, however, and except for 1966 is within the normal limits of sampling error. The gap between the surveyed and actual vote was 2.7 percent in 1964, 4.1 percent in 1966, and 0.9 percent in 1970. Richard Rose, "Britain: Simple Abstractions and Complex Realities" in *Electoral Behavior: A Comparative Handbook*, ed. Richard Rose (New York: Free Press, 1974), p. 495, points out that the overall Butler-Stokes errors in estimating the gap for Britain (exclusive of Northern Ireland) in 1964 and 1966 were 3.0 percent and 4.8 percent respectively. "While the degree of error involved in a sample survey may be crucial in forecasting which party wins a highly competitive British election, it is of very limited concern in an analysis of major social characteristics of voters." Similar reasoning would seem to apply when a major attitudinal characteristic of voters is added to the analysis. The results for the actual population of England include the nonwhite voters, which is thought to shift the results in the direction of Labour at all three elections by a small amount. Nonwhites are not included in the survey results. Since the N for the regression runs was reduced considerably, especially in 1970, by pairwise deletion of cases, it is noteworthy that the surveyed vote for the multivariate analysis is as follows:

	1964	1966	1970
Labour	55.9%	57.2%	48.5%
Conservative	44.1	42.8	51.5
	100.0%	100.0%	100.0%
	(N = 849)	(N = 1012)	(N = 335)

[19] On the use of dummy variables, see Jacob Cohen, "Multiple Regression as a General Data-Analytic System," *Psychological Bulletin*, 70 (1968), 426–43; Daniel B. Suits, "Use of Dummy Variables in Regression Equations," *Journal of the American Statistical Association*, 52 (December 1957), 548–51; and Mark N. Franklin, *Daedal: A Data Archiving, Editing, Describing and Analyzing Language* (Edinburgh: University of Edinburgh, 1973), pp. 89–90. Treating ordinal-level variables as if they were interval is discussed in Sanford Labovitz, "Some Observations on Measurement and Statistics," *Social Forces*, 46 (December 1967), 151–60; Sanford Labovitz, "The Assignment of Numbers to Rank Order Categories," *American Sociological Review*, 35 (June 1970), 515–24; H. T. Reynolds, "Ordinal Partial Correlation and Causal Inferences," in *Measurement in the Social Sciences: Theories and Strategies*, ed. Hubert M. Blalock, Jr. (Chicago: Aldine, 1974), pp. 399–423; H. T. Reynolds, "On Testing Causal Models with Ordinal Data" (University of Delaware, mimeographed, 1972).

[20] Labovitz, "Some Observations on Measurement and Statistics," pp. 156–58; Reynolds, "Ordinal Partial Correlation and Causal Inferences," p. 417.

One aspect of the multivariate analysis is sufficiently unconventional to warrant special comment. This is the treatment of the dependent variable, the vote, as a dichotomous variable, scored 0 for Labour voters and 1 for Conservative voters. This procedure enables the researcher to assess the impact of the independent variables as they affect the mean of the dependent variable. The higher the mean, the more people who voted Conservative in the two-party division of the vote. Furthermore, since the vote is coded 0–1, these means can be directly translated into percentages, i.e., a mean vote of .49 is equivalent to 49 percent of the sample voting for the Conservatives and 51 percent for Labour.[21]

The regression equations focus on the unstandardized regression coefficients (*b* weights) of the independent variables and how they affect the mean scores of the dependent variable, the vote. The regression coefficients tell what change in the dependent variable is produced by a unit change in each of the independent variables.[22] For example, the *b* weight of the variable denoting opinion on immigration shows what change is produced in the mean Conservative vote as one moves across the four categories of immigration opinion. Assuming linearity, one can interpret this *b* as the average change in the value of the vote variable as one moves from one category of the independent variable to another.

Since all of the relationships are not perfectly linear, however, the *b* weights are best interpreted as the approximate equivalents of the actual percentage differences among the categories.[23] Once one obtains this average effect for the categories of the independent variable, one can calculate the total effect of the variable by standardizing it.[24] In the case of the immigration opinion variable, the resulting figure indicates the effect, in percentage terms, of hostility toward immigrants on the two-party vote in a particular election.[25]

The recent outpouring of scholarly literature on issue voting points up many of the difficulties of attempting to link public opinion on policy issues with individual voting behavior. For an issue to have an impact on the voting behavior of the mass public, it must be salient

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[21] Other research employing this procedure includes Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March 1966), esp. 27–28; Jae-on Kim, John R. Petrocik, and Stephen N. Enokson, "Voter Turnout Among the American States: Systemic and Individual Components," *American Political Science Review*, 69 (March 1975), 107–23. This method is also the equivalent of the dichotomization of the dependent variables employed in Multiple Classification Analysis (MCA). See Frank M. Andrews, James N. Morgan, and John A. Sonquist, *Multiple Classification Analysis: A Report on a Computer Program for Multiple Regression Using Categorical Predictors* (Ann Arbor, Mich.: Institute for Social Research, University of Michigan, 1967). Fred Kort argues that discriminant function analysis is preferable to multiple regression for a dichotomous dependent variable. See "Regression Analysis and Discriminant Analysis: An Application of R. A. Fisher's Theorem to Data in Political Science," *American Political Science Review*, 67 (June 1973), 555–59. His example, however, belies his argument. See also the exchange between Peter H. Lemieux and Kort in the "Communications" section of the *American Political Science Review*, 68 (March 1974), 202–05.

[22] Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964), p. 52.

[23] The *b* weights and the actual averages of the percentage differences by crosstabulation were calculated for the bivariate relationship of opinion and vote, controlling for each of the party perception categories in each year. The comparative results were as follows (*b* weight presented first):

Party More Likely to Keep Immigrants Out			
Labour	–.11 (–.11)	–.14 (–.16)	–.05 (–.06)
No Difference	.01 (.02)	.01 (.01)	–.02 (–.04)
Conservatives	.06 (.06)	.08 (.06)	.11 (.11)

This procedure, unlike that using dummy variables, does not indicate which categories make the most difference in the scores of the dependent variable.

[24] This is the method used by Stokes to calculate attitudinal components of electoral decision in "Some Dynamic Elements of Contests for the Presidency." See esp. 27–28.

[25] In a multiple regression equation, the *b* weights and *beta* weights of all the independent variables can be compared to see which ones make more of an impact on the dependent variable net of the others. A small *b*, of course, would indicate small percentage differences among the categories and a conclusion of no effect for that particular variable. Contrariwise, a large *b* can be interpreted as signifying large percentage differences in Conservative vote among the categories and a conclusion of substantial impact of that variable on voting behavior. The *beta* weights of all the independent variables can be used to calculate the net effects of each variable in a path analysis. If the *b* coefficient is 1.5 times its standard error of estimate, a statistically significant path exists between two variables. This is a liberal standard for determining the significance of a path. Since this is an exploratory study incorporating more variables than previously examined in studies of this kind, it is more important to include any possibly important variables in the equations than to exclude variables in the interest of a rigorously specified theory. In this case, no rigorously specified theory would justify more stringent standards for including variables. Using only those variables found to be significantly related, one then calculates a second set of equations. The values from this second

to the voters, their opinions must be skewed toward one side of the issue, and they must view one party as more likely to enact their preferred policy than the other (in two-party competition).[26] However, even if all of these conditions are met, other considerations can prevent issue voting. Traditional party affiliations or the personal attractiveness of individual candidates may interfere with issue voting. The party preferred on one issue may differ from that preferred on other issues, which moderates the effect of that particular issue and perhaps cancels it.[27] Group attachments or social surroundings may be stronger influences on voting choice than policy issues, even when these issues otherwise meet the conditions for issue voting. Most explanations of voting behavior by positions on policy issues compete with explanations on grounds of group membership, contextual influence, and long-term partisan commitments rather than attempting to integrate all of these components into an overall framework of voting choice.[28]

This analysis presents a partial corrective to the above problems associated with the issue voting approach. While not pretending to account for all factors influencing voting choice, especially long-term partisan commitments and the effects of other issues, it does control for the effects of group membership (individual socioeconomic attributes), social context, and the appeals of parliamentary candidates on the immigration issue. Following a model outlined by Matthews and Prothro, it attempts to assess what contribution opinion on the immigration issue makes to voting behavior net of these other variables.[29] Many of these variables, of course, affect both opinion on immigration and voting choice. The applicable model is stated in Figure 1. This multivariate approach to issue voting provides at least a small step forward toward integrating the heretofore competitive explanations of voting.

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set provide estimates of the path coefficients. This procedure enables one to assess both the direct and indirect contributions of a particular variable to variation in the dependent variable. Any masking of the effects of a variable by its relation to another variable will thus be revealed. In other words, the net effect coefficients can be considered partial correlations, with multiple controls for the effects of the other independent variables. See David Knoke, "A Causal Model for the Political Party Preferences of American Men," *American Sociological Review,* 35 (December 1972), 679–89; Giacomo Sani, "Determinants of Party Preference in Italy: Toward the Integration of Complementary Models," *American Journal of Political Science,* 18 (May 1974), 315–29.

[26] Butler and Stokes, *Political Change in Britain,* 2nd ed., pp. 287–95. The first development of these conditions was in Angus Campbell et al., *The American Voter,* pp. 168–87.

[27] Richard A. Brody and Benjamin I. Page, "Comment: The Assessment of Policy Voting," *American Political Science Review,* 66 (June 1972), 454–55.

[28] Two exceptions to this generalization are Fishbein and Coombs, "Basis for Decision: An Attitudinal Approach Toward an Understanding of Voting Behavior," and David Knoke, "A Causal Synthesis of Sociological and Psychological Models of American Voting Behavior," *Social Forces,* 53 (September 1974), 92–101.

[29] This model is a variation of the one employed by Donald R. Matthews and James W. Prothro, *Negroes and the New Southern Politics* (New York: Harcourt, Brace and World, 1966), pp. 27, 323. See also Sani, "Determinants of Party Preference in Italy: Toward the Integration of Complementary Models"; Butler and Stokes, *Political Change in Britain,* 2nd ed., pp. 287–95.

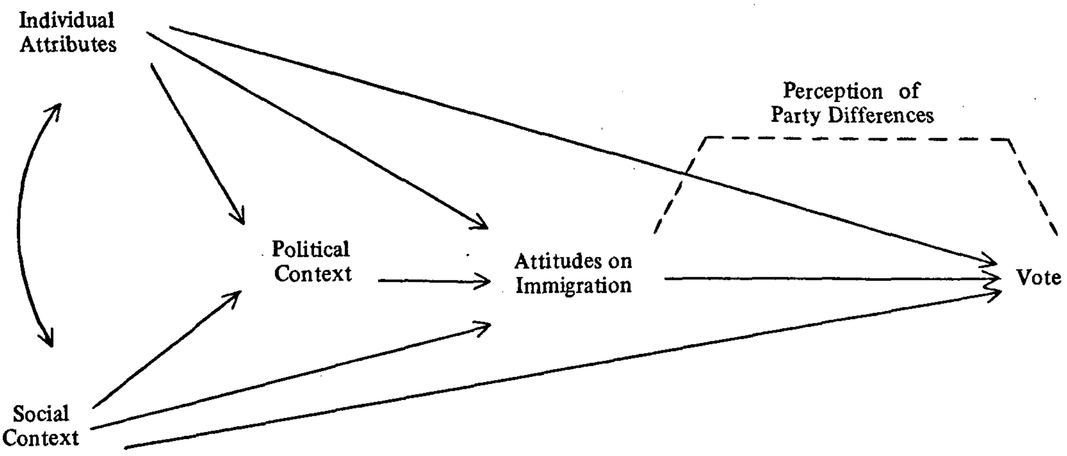

Figure 1. Model of Variables Affecting Voting Behavior

The model employed is developmental. Different opinions on immigration are generated by individual socioeconomic attributes, social context, and political environment. Once formed, these opinions may, because of their interaction with individual perceptions of party differences on immigration policy, affect voting behavior free of any effects on the vote caused by background variables.[30] That is the major hypothesis tested in this paper. The results should show whether issue voting on immigration policy has taken place in England.

The Findings

Opinions on Immigration Policy. We have assessed opinions on immigration in this analysis by combining two of the questions that Butler and Stokes asked in their 1964, 1966, and 1970 studies. These questions are as follows:

1. Do you think that too many immigrants have been let into this country or not?
2. How strongly do you feel about this—very strongly, fairly strongly, or not very strongly?

The pattern of responses to these questions is shown in table 1. These questions were combined because the first question seemed to be phrased so as to elicit a negative response. In addition, with the responses so highly skewed in one direction, one could make only a very limited analysis without lowering the N beyond acceptable limits. Therefore, it seems both more valid and more convenient for the data analysis to reconstruct the responses to the questions on immigration.

Combining these two questions on immigration yields four categories of opinion on an ordinal scale from favorable to strongly anti-immigrant.[31] The distribution of these opinions within the English white population at the time of the elections of 1964, 1966, and 1970 is shown in Table 2. A glance at Table 2 shows that the distributions of opinions on immigration did not vary over the course of the three elections. Almost identical percentages of the population were found in each category of opinion from election to election. These data bespeak a remarkable stability in the opinions of the British electorate on the immigration issue.[32] Despite the different political circum-

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[30] On developmental models, see Blalock, *Causal Inferences in Nonexperimental Research*, pp. 85–87; Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September 1969), 860–62. The causal sequence of the variables reflects a temporal order, that is, a person's social context and individual attributes are temporally antecedent to his political context, which in turn is assumed to be antecedent to his opinion on immigration and his vote. The individual attribute variables are also modeled according to an assumed temporal order in the life of an individual:

1.	Age cohort Religion	Church attendance	Kind of school attended	Education
2.	Social class	Income	Subjective social class	

The categories of the individual attribute variables are as follows:

1. Age cohort (1 = less than 36 years, 2 = 36–50 years, 3 = 51–65 years, 4 = 66 years+)
2. Religion 1 (0 = unbelievers, 1 = Anglicans).
3. Religion 2 (0 = unbelievers, 1 = other Protestants)
4. Religion 3 (0 = unbelievers, 1 = Roman Catholics)
5. Church attendance (1 = never–unbelievers, 2 = never–believers, 3 = less than once a year, 4 = several times a year, 5 = several times a month)
6. Kind of school (1 = elementary, secondary modern, 2 = grammar, 3 = public school)
7. Education (0 = left school at minimum age, 1 = stayed in school beyond minimum age)
8. Social class (1 = D, 2 = C2, 3 = C1B, 4 = C1A, 5 = B, 6 = A)
9. Income (1 = under £350, 2 = £350–549, 3 = £550–749, 4 = £750–1199, 5 = £1200+)
10. Subjective social class (0 = working class, 1 = middle class).

[31] The two questions were combined. Those who felt very strongly that there were too many immigrants in the country were assigned to the "strongly anti-immigrant" category. Those who felt only fairly strongly that there were too many immigrants in the country were assigned to the "moderately anti-immigrant" category. Those who thought that there were too many immigrants in the country but did not feel strongly about it were assigned to the "weakly anti-immigrant" category. Finally, those who did not think that there were too many immigrants in the country were assigned to the "favorable" category. There is a discrepancy in the N of question 2 in Table 1 and the N in Table 2 because in 1964 and 1966 people who did not think that there were too many immigrants in the country were not asked how strongly they felt about this issue. Since by themselves these people constitute the "favorable" category in Table 2, the N is larger for that table.

[32] The stability of findings in electoral situations is similar to that in nonelectoral situations found by Donley T. Studlar, "British Public Opinion, Colour Issues, and Enoch Powell: A Longitudinal Analysis," *British Journal of Political Science*, 4 (July 1974), 371–81. Since electoral campaigns present the ordinary elector with stimuli that may cause him to behave differently than he would in electoral situations, the persistence of attitudes on immigration is worthy of emphasis. On the differential stimuli of electoral campaigns, see Berelson, Lazarsfeld, and McPhee, *Voting*, pp. 291–96; and W. L. Miller and M. Mackie, "The Electoral Cycle and the Asymmetry of Government and Opposition Popularity: An Alterna-

stances characterizing each of the three elections, the overall structure of attitudes toward immigrants within the English population did not change.[33]

Immigration as a political issue was at its most partisan during the 1970 campaign. Enoch Powell, then a right-wing Conservative MP and preeminent spokesman for the anti-immigration sentiment in the country, was a center of controversy during the campaign. Powell's position on immigration was excoriated by the Labour Minister of Technology, Anthony Wedgwood Benn, as being similar to the racial ideas of the Nazis. Powell himself made two heavily publicized speeches on the topic. Although there was considerable pressure upon Edward Heath, leader of the Conservatives, to repudiate Powell, Heath stepped gingerly around the subject. Powell himself urged the electorate to vote for the Conservative party only two days before the election.[34]

Further data, available only for the 1970 sample, show the structure of opinions on immigration in greater detail. Table 3 indicates that Enoch Powell was indeed the people's champion on the immigration issue. The policy of the sitting Labour government in 1970 was closest to the third response in Table 3. Despite some differences in emphasis, neither the La-

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tive Model of the Relationship Between Economic Conditions and Political Popularity," *Political Studies*, 21 (September 1973), 263–79.

[33] Since the surveys under examination are samples of the English electorate rather than panels, these figures do not mean that the direct movement of individual respondents is necessarily indicated by the overall figures.

[34] Accounts of the 1970 campaign and of the relative attention given in it to the immigration issue are contained in Butler and Pinto-Duschinsky, *The British General Election of 1970*, ch. 7, and George Gale, "The 1970 Election Campaign" in *Powell and the 1970 Election*, ed. Wood, pp. 50–82.

Table 1. Opinions on Immigration, 1964–1970

1. Do you think that too many immigrants have been let into this country or not?[a]

	1964	1966	1970	(1970)
Yes	81.6%	81.5%	83.4%	(86.6%)
No	13.0	13.9	12.7	(9.3)
Don't know	5.4	4.6	3.9	(4.1)
	100.0	100.0	100.0	(100.0)
(N)	(1559)	(1618)	(1062)	(798)

2. How strongly do you feel about this–very strongly, fairly strongly, or not very strongly?

	1964	1966	1970
Very strongly	54.3%	55.5%	51.9%
Fairly strongly	32.3	31.9	33.5
Not very strongly	13.4	12.6	14.6
	100.0	100.0	100.0
(N)	(1270)	(1332)	(1018)

[a] In 1970 two different questions were asked. The first column of figures in the table represents the combined responses to the two questions. The figures in parentheses are the responses to the question as it was asked in 1964 and 1966. The other question and the distribution of responses was as follows:

Do you think that the number of immigrants that have been let into this country is all right, or not? (Have there been too many or too few let in?)

Yes, all right	22.0%
No, too many	73.9
No, too few	1.1
Don't know	3.0
	100.0
(N)	(264)

In order to make the answers to this question comparable to those of the other question, the first and third categories of response were combined.

bour party manifesto nor the Conservative party manifesto differed appreciably from that policy.[35] The policy preferences of the electorate, however, were more strongly anti-immigrant than either current or prospective government policy—72 percent wanted at least to cut off all further immigration (presumably including dependents of immigrants already in Britain). The most prominent advocate of such a policy was Enoch Powell, who had gone even further and urged an active policy of repatriation for immigrants. Over 75 percent of the public knew something about Powell's proposals. Given the usual indifference of the general public to political events, this is remarkable testimony to Powell's persistent advocacy on the topic.[36] Even more remarkable, how-

Table 2. Opinions on Immigration, 1964–1970

	1964	1966	1970
Favorable	13.8%	14.6%	13.3%
Weakly anti-immigrant	11.5	10.5	9.8
Moderately anti-immigrant	27.8	27.2	28.3
Strongly anti-immigrant	46.9	47.7	48.6
	100.0	100.0	100.0
(N)	(1468)	(1541)	(1016)

Table 3. Policy Preferences on Immigration, 1970 Only

1. Which of these statements comes closest to what you yourself feel should be done about immigrants?
2. Which of these statements comes closest to his [Powell's] position?

	Own Position	Powell's Position
Assist in sending immigrants home	22.3%	76.7%
Stop further immigration but allow immigrants already here to stay	49.3	19.0
Allow in immediate families of immigrants already here and a few skilled workers only	21.0	2.1
Allow in new workers and their families	3.8	0.2
Allow free entry	1.2	0.1
Don't know	2.4	1.9
	100.0	100.0
(N)	(1058)	(797)

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[35] The 1970 manifestoes of the two major parties read as follows:

Conservative Party: Good race relations are of immense importance. We are determined that all citizens shall continue to be treated as equal before the law, and without discrimination.... Local authority services are under great strain in many of the towns and cities where large numbers of immigrants have settled. We believe that additional funds should be made available to these local authorities in order that they can deal with these problems effectively without placing heavy burdens on their ratepayers.

We will establish a new single system of control over all immigration from overseas.... We believe it right to allow an existing Commonwealth immigrant who is already here to bring his wife and young children to join him in this country. But for the future, work permits will not carry the right of permanent settlement for the holder or his dependants....

These policies mean that future immigration will be allowed only in strictly defined special cases. There will be no further large-scale permanent immigration.

We will give assistance to Commonwealth immigrants who wish to return to their countries of origin, but we will not tolerate any attempt to harass or compel them to go against their will.

Labour Party: With the rate of immigration under firm control and much lower than in past years, we shall be able still more to concentrate our resources in the major task of securing good race relations. The urban programme includes help to areas of high immigrant population, where special social needs exist. The Race Relations Act has outlawed incitement to racial hatred and discrimination in housing, employment and credit facilities.... We now propose to review the law relating to citizenship and to give the Race Relations Board powers of discretion in taking up complaints.

[36] Examples of mass detachment from political events are given in Butler and Stokes, *Political Change*

ever, is the accuracy with which the general public perceived Powell's proposals on immigration. Of those who claimed to have heard something about Powell's proposals, almost 96 percent had at least a partially accurate perception of his position. Although they lag somewhat behind Powell's position, by and large the electorate's opinions are closer to Powell's than to the actual position of either the Labour or Conservative party.

Perception of Party Differences. For an issue of public policy to have an effect on mass voting behavior, voters must perceive the positions of major political parties as sufficiently different as to indicate that the winning party would take action on the issue that the other would not take. Perception of party differences, then, is an intervening variable between opinion on an issue and voting behavior. If voters perceive no differences between parties, then issue voting cannot take place, no matter how widespread and intense public opinion on the issue may be.[37]

It is important to note that the perceived differences between the parties may not be equivalent to the objective differences revealed by a thorough, dispassionate analysis. The voters' mental image of each party's position is the significant factor. Since most voters do not search systematically for information on party differences, even on issues which are salient to them, one would not necessarily expect their mental images of party positions to be closely related to the parties' real positions.[38] Moreover, once these images are formed, they can linger and be very difficult to change.[39]

Butler and Stokes contend that the immigration issue failed to affect voting choice in the 1964 and 1966 elections because party positions on immigration were not widely perceived as different. Hence the issue was one of "high potential"; if a widespread differentiation between party positions could have been made, "the issue could have altered the party balance sharply."[40] Their most recent analysis indicates that this is exactly what happened by the time of the 1970 election, even though the actual Conservative and Labour positions on immigration policy were not radically different in substance. Butler and Stokes attribute this change in the perceptions of party positions on immigration to Enoch Powell's success in associating the Conservatives with opposition to immigration in the voters' minds.[41] As has been demonstrated, certainly the public's attitudes were closer to Powell's position than to the actual positions of either major party.

What, then, are the mass perceptions of Conservative and Labour positions on immigration policy? In each of the three elections under analysis respondents were asked "Which party is more likely to keep immigrants out, the Conservatives or Labour, or don't you feel there is much difference between them on this?" The data for the elections of 1964 and 1966 support the Butler-Stokes contention that the public as a whole failed to perceive differences between the two parties on immigration policy.[42] Slightly more people saw the Conservatives as more likely to keep immigrants out than Labour. What is more noteworthy, however, is that a majority of respondents—56 percent in 1964 and 61 percent in 1966—could

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in Britain, 2nd ed., pp. 22–23; and Abrams and Rose, *Must Labour Lose?* pp. 73–74. The argument is familiar, of course, to those who have read the SRC studies of American elections. See especially Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," in Campbell et al., *Elections and the Political Order*, pp. 194–211.

[37] See the discussion of the conditions for issue voting in Butler and Stokes, *Political Change in Britain*, 2nd ed., pp. 287–95.

[38] Evidence supporting this contention for elections in the United States is presented in Berelson, Lazarsfeld, and McPhee, *Voting*, pp. 215–33, and Campbell et al., *The American Voter*, pp. 179–87. British findings on the same point are available in Benney, Gray, and Pear, *How People Vote*, pp. 140–46; Milne and Mackenzie, *Marginal Seat, 1955*, pp. 117–21; and Pulzer, *Political Representation and Elections*, pp. 114–15.

[39] See Stokes, "Some Dynamic Elements of Contests for the Presidency."

[40] Butler and Stokes, *Political Change in Britain*, 2nd ed., pp. 303–05.

[41] Ibid., pp. 307–08.

[42] Full tabulations are available on request. Part of the data on party differences for 1964 and 1966 is simulated. This was necessitated by the original Butler-Stokes questionnaire for those years, which directed that only those who thought that there were too many immigrants in the country were to be asked which party was more restrictive. Those respondents favorable toward immigrants were not asked for their perceptions of party differences. A continuation of this procedure here would result in undesirable complications since perception of party differences is a necessary link between opinion on immigration and voting behavior. It was assumed that the closest available approximation to the actual distribution of "favorable" respondents over the perception categories in 1964 and 1966 would be the 1970 distribution. Accordingly, respondents favorable to immigrants in the 1964 and 1966 surveys were randomly assigned to one of the perception categories on the basis of the distribution of favorable respondents over the perception categories in the 1970 data.

see no appreciable difference between the parties on this issue. In the eyes of the public, immigration control was not a partisan political issue in the mid-1960s.

By 1970, however, the situation had changed considerably. Fifty-seven percent of those surveyed now saw the Conservatives as the more restrictive party. The spread between those choosing the Conservatives and those selecting Labour as more restrictive leaped from 14 percent in 1966 to almost 50 percent in 1970. Even more importantly, the percentage of people seeing no difference between the parties dropped to 35 percent. In 1970 the British electorate did perceive clear differences between the parties on immigration policy.

Table 4 brings these differences into sharper focus. When asked to choose which among five alternative policies came closest to the positions of each of the two major parties, respondents tended to view Conservative policy as more restrictive. It has already been pointed out that the substantive immigration policy of both parties in 1970 most nearly approximated the third response in the table. Yet 57 percent of respondents thought that the Conservative position was closest to one of the first two categories, i.e., they saw Conservative policy as more restrictive than it actually was. On the other hand, only eight percent saw it as less restrictive than it really was (responses four and five). More people perceived the Labour position correctly (29 percent in category three) than they did the Conservative position (18 percent). Only 24 percent, however, saw Labour policy as more restrictive than it really was, while 34 percent saw it as less restrictive.

The Labour party was not perceived to be "soft on immigration" in an absolute sense. Fifty-three percent of the respondents placed its policy in one of the first three categories. The Conservative party, however, was seen as more restrictive. Fully 76 percent of those interviewed thought that Conservative policy fit one of the first three responses. On a comparative basis, the English public in 1970 saw the Conservatives as the party more likely to keep immigrants out.

Combining these findings with those presented previously on public perceptions of Enoch Powell's proposals on immigration, one can conclude that Powell was the major influence in separating the party policies in voters' minds.[43] Most of the electorate had heard of Powell, were familiar with his proposals on immigration, and were closer in their opinions to Powell than to the actual positions of the Labour and Conservative parties. The events of the parliamentary campaign in 1970 worked to associate Powell's position with the Conservative party, however much the party may have balked at the notion.[44] The Labour party had shown itself quite willing to put stringent controls on immigration by passage of the Kenyan Asians Act in 1968. Despite this episode, the electorate in 1970 had swung radically from seeing no appreciable differences between the parties on immigration policy in 1964 and 1966 to seeing the Conservatives as the more restrictive party. It is difficult to

Table 4. Perception of Party Positions on Immigration, 1970 Only

1. Which of these statements do you think comes closest to the Labour party's position?
2. Which of these statements do you think comes closest to the Conservative party's position?

	Labour	Conservative
Assist in sending immigrants home	4.0%	22.0%
Stop further immigration but allow immigrants already here to stay	19.7	35.4
Allow in immediate families of immigrants already here and a few skilled workers only	29.0	18.7
Allow in new workers and their families	23.3	5.8
Allow free entry	10.2	2.2
Don't know	13.8	15.9
	100.0	100.0
(N)	(530)	(533)

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[43] Butler and Stokes, *Political Change in Britain*, 2nd ed., pp. 307–08.

[44] Butler and Pinto-Duschinsky, *The British General Election if 1970*, pp. 159–63; Gale, "The 1970 Election Campaign," pp. 66–82.

argue with Butler and Stokes's contention that Powell was the reason.[45]

Opinion on Immigration and Perception of Party Differences. Whatever influence perception of party differences exercises over the voting decision depends on its interaction with opinion on immigration. By itself, perception of party differences has no independent influence on voting behavior. The question arises, do individuals perceive the parties differently because of their differing substantive opinions on the issue? An answer is provided in Table 5. The results indicate that one's immigration opinion has very little influence on his perception of differences in party policies. Extremely weak relationships between the two existed in 1964 and 1966, and no statistically significant relationship existed in 1970. In short, an individual's perception concerning which party is more restrictive does not depend on his own opinion about immigration policy, i.e., strongly anti-immigrant people do not disproportionately see the Conservative party as more favorable to their cause, etc. However, a larger percentage of people who are strongly anti-immigrant choose one of the two major parties as being more likely to keep immigrants out than people in the other categories of opinion on this issue. In other words, strongly anti-immigrant respondents are more likely to see differences between the party positions, even if they do not agree as to which party is the more restrictive. This is further indication of the interaction of opinions and perceptions of party differences.

The fact that opinion on immigration bears only a weak relationship to perceptions of party differences on this issue does not neces-

Table 5. Perception of Party Positions on Immigration by Opinion on Immigration, 1964–1970

Party More Likely to Keep Immigrants Out	Opinion on Immigration: Favorable	Weakly Anti-Immigrant	Moderately Anti-Immigrant	Strongly Anti-Immigrant
			1964	
Labour	19%	14%	16%	20%
No difference	61	65	60	49
Conservative	20	21	24	31
	100	100	100	100
N = 1464	(203)	(168)	(405)	(687)
		$p < .001$	$r = .050$ $R^2 = .002$	
			1966	
Labour	10%	11%	9%	16%
No difference	67	69	65	54
Conservative	23	20	26	30
	100	100	100	100
N = 1539	(226)	(161)	(419)	(734)
		$p < .001$	$r = .012$ $R^2 = .000$	
			1970	
Labour	7%	6%	6%	9%
No difference	40	44	38	30
Conservative	53	50	56	61
	100	100	100	100
N = 1002	(135)	(98)	(283)	(487)
		p = n.s.	$r = .034$ $R^2 = .001$	

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[45] Fox finds that by 1974 the electorate was once more confused about which party was more restrictive. This is not surprising, since the Conservative government admission of refugee Asians from Uganda in the fall of 1972 served to alter the restrictive image Powell had given the party. See Fox, "Attitudes to Immigration: A Comparison of Data from the 1970 and 1974 General Election Surveys," 176–78. On the Uganda Asians episode, see Derek Humphry and Michael Ward, *Passports and Politics* (Harmondsworth, Middlesex: Penguin Books, 1974).

sarily disprove the hypothesis of a linkage between immigration opinion and voting behavior. Even though people of all persuasions on immigration policy saw differences between the parties almost identically, their actions (voting behavior) based on these perceptions would be expected to differ. For example, those strongly anti-immigrant who saw the Conservatives as the more restrictive party should be more likely to vote Conservative than those with a similar idea of party differences but whose own opinion is more favorable toward immigration. Strongly anti-immigrant respondents who thought Labour more restrictive would be expected to shift their votes toward Labour. The relationship of perception of party differences to vote depends on what meaning this perception has for an individual in the light of other attitudes.

Immigration Policy Voting. If issue voting occurs, then individuals who are higher on the immigration opinion scale (more anti-immigrant) and who perceive one party as more likely to keep immigrants out would vote for that party in greater numbers than those lower on the opinion scale who see that party as more restrictive. In statistical terms, if issue voting occurs, the regression coefficient (*b* weight) of opinion with vote within a category of perceived party differences will be high and statistically significant. For those who see no difference between the parties on the immigration issue, the relationship between opinion and vote should be weak or nonexistent. One can draw conclusions about the degree of issue voting occurring in the population by calculating the percentage of respondents who do perceive party differences, the percentage who do not perceive such differences, and the strength of the opinion-vote relationships in each group for each category. Which party benefits more from issue voting can be assessed by noting the relative strengths of the relationships, their direction, and the percentage of respondents in each of the categories of perceived party differences.

Before proceeding with these multivariate analyses, however, it is worth noting the bivariate relationships between opinion on immigration and voting behavior. There are no zero-order relationships between immigration opinion and vote in 1964 and 1966, and only a weak one in 1970 (Table 6). At this point it might appear that no issue voting on the immigration issue occurred in 1964 and 1966, and very little in 1970.[46]

Table 7 shows how controlling for perceived party differences changes the relationships between opinion and vote. In all three elections, those who perceived the Labour party as more anti-immigrant showed a relatively high propensity to vote Conservative because of their own less hostile opinions on immigration. Although the N's in some categories are relatively small in all three years, especially in 1970, the stability of the correlation coefficients strengthens these findings. Labour received a much higher proportion of strongly anti-immigrant votes than other votes, especially in 1964 and 1966.

Those who perceived the Conservatives as the party more likely to keep immigrants out also voted on the basis of this issue. More of these voted Conservative because of their hostility toward immigrants. This relationship between opinion and vote reached its zenith in 1970, the point at which its counterpart relationship among Labour perceivers was at its nadir. Table 7 also bears out the third contention of the argument about the interaction of opinion and perceived party differences, namely that among those who see no difference between the parties on the immigration issue there should be no significant relationship between opinion and vote. This was the case in all three elections.

Even though the bivariate relationships between immigration opinion and voting behavior did not indicate issue voting, the introduction of perception of party differences as an intervening variable presented contingency-table evidence of considerable issue voting, even in 1964 and 1966. The case for issue voting on immigration, however, has not yet been convincingly made. Before one can argue that opinion on immigration did lead people to vote for a particular party, one must examine alternative explanations of voting behavior. This can be done by incorporating elements of other explanations into a developmental model relating background variables, opinion on immigration, perception of party differences, and voting behavior, as outlined previously. Variables which affect both immigration opinion and voting behavior can be controlled, and the net effect of opinion on vote assessed. Such an analysis more accurately establishes the existence of issue voting on immigration and its extent.

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[46] Page and Brody suggest that this test is sufficient to establish that no issue voting is taking place; however they do note that one should examine perceptions before drawing a final conclusion. They do not, however, consider that other variables should also be controlled in a multivariate design. See Benjamin I. Page and Richard A. Brody, "Policy Voting and the Electoral Process: The Vietnam War Issue," *American Political Science Review*, 61 (September 1972), 982.

The significant background variables are those which, according to a separate analysis, are related to immigration opinion in a particular election. Individual socioeconomic attributes, social context, and appeals of anti-immigrant parliamentary candidates which affected opinion on immigration are placed in a multiple regression equation along with opinion, and voting choice is regressed on them. The interaction of opinion and party differences is noted by running separate regressions for each party difference category at each election.

The summary results of the full multiple regression equations and the path analyses they generate are presented in Table 8. Only those variables found to be significantly related, directly or indirectly, to opinion on immigration in a particular year are included.[47] Look-

Table 6. Vote by Opinion on Immigration, 1970

Vote	Opinion on Immigration			
	Favorable	Weakly Anti-Immigrant	Moderately Anti-Immigrant	Strongly Anti-Immigrant
Labour	57%	59%	49%	43%
Conservative	43	41	51	56
	100	100	100	100
N = 738	(94)	(69)	(214)	(362)
		$p < .05$	r = .109	$R^2 = .012$

Table 7. Percent Conservative Vote by Opinion on Immigration and Perception of Party Differences, 1964–1970

Opinion on Immigration	Party More Likely to Keep Immigrants Out		
	Labour	No Difference	Conservative
		1964	
Favorable	54%	38%	50%
Weakly anti-immigrant	44	53	49
Moderately anti-immigrant	14	48	69
Strongly anti-immigrant	21	46	67
r =	–.264	.019	.122
N =	213	615	305
		1966	
Favorable	61%	39%	55%
Weakly anti-immigrant	25	39	40
Moderately anti-immigrant	19	40	66
Strongly anti-immigrant	14	37	72
r =	–.346	–.018	.168
N =	158	694	312
		1970	
Favorable	40%	57%	34%
Weakly anti-immigrant	23	38	46
Moderately anti-immigrant	33	46	56
Strongly anti-immigrant	21	46	68
r =	–.129	–.040	.234
N =	58	243	427

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[47] The full correlation matrices are available on request from the author.

ing at these results first by year, one can see that the observations regarding the linkage between issues and votes made in the immediately preceding section are borne out by this more complex analysis. Holding constant all relevant individual attribute and social context variables for the 1964 election, one still finds a significant relationship between opinion on immigration and voting behavior for those who saw either the Labour party or the Conservative party as more likely to keep immigrants out. However, there was no relationship between immigration opinion and vote for those who failed to see a difference between the parties. The relationship was slightly stronger for those who perceived the Conservatives as more restrictive, and the directions of the two relationships were reversed as expected. An examination of the regression coefficients for these two categories shows that the differences in the vote across the opinion categories in the "Conservative party best" group averaged nine percent while the differences in the "Labour party best" group averaged six percent. Since the "Conservative best" group was somewhat larger than the "Labour best" group, one cannot simply assume that these effects canceled each other out, despite the fact that they were of similar magnitude and pointed in opposite directions. If one considers all respondents, even those who saw no difference between the parties, one can calculate that the Conservatives had an average net gain on the immigration issue of 1.3 percent across the categories of opinion in 1964.[48] This means that the total net effect of hostility toward immigrants in 1964 amounted to a 2.7 percent gain for the Conservatives. Since this 2.7 percent is less than sampling error, one cannot confidently say that opinion on immigration had any effect on the overall direction of voting behavior in 1964. Even though the Conservatives had a slight advantage over Labour in being perceived as the more restrictive party, this advantage was not

Table 8. The Impact of Opinions on Immigration on Vote Within Categories of Perception of Party Positions, 1964–1970[a]

	Party More Likely to Keep Immigrants Out					
	Labour		No Difference		Conservative	
	Full Model	Reduced Model	Full Model	Reduced Model	Full Model	Reduced Model
	1964					
	–.063**	–.066**	.017		.087**	.093**
	(–.169)	(–.178)	(.037)		(.176)	(.188)
R² (all variables)	.310	.284	.325	.318	.277	.235
	1966					
	–.048**	–.048**	–.020		.091**	.098**
	(–.124)	(–.124)	(–.022)		(.186)	(.200)
R² (all variables)	.429	.419	.221	.185	.273	.253
	1970					
	–.088		–.028		.076**	.074**
	(–.201)		(–.065)		(.164)	(.160)
R² (all variables)	.252		.142	.112	.136	.121

[a]The full model includes all variables with previously found relationships to opinion on immigration. The reduced model includes only those variables from the full model found to be directly or indirectly related to the vote within each category of perceived party differences. The first value is the *b*, the unstandardized partial regression coefficient, for opinion on immigration. The second value, in parentheses, is its *beta*, the standardized partial regression coefficient. A double asterisk marks the significant values ($b \geqslant 1.5$ standard errors). More detailed tables are available from the author on request.

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[48] This average net effect for the entire sample in each year was calculated in the following manner. The *b* weight for each perception category was multiplied by the N in that category. Since the "no difference" *b* weights were not significant, 0 was assigned as the weight for this group of respondents. The results of these three multiplications were added together. This means that the figure from the Labour perceivers, with a negative *b* weight, was subtracted from the figure from the Conservative perceivers, with a positive *b* weight. This number was then divided by the total N in the sample for that year, resulting in the average net effect for the entire sample. The average net effect was standardized according to the formula provided by Stokes, "Some Dynamic Elements of Contests for the Presidency," 27–28, to yield the total net effect.

pervasive enough in the population to make a significant difference in drawing votes.[49]

The results for the 1966 election, presented in Table 8, are similar to those just described for 1964. Those who failed to perceive differences in the party positions on immigration did not vote on the basis of this issue. Those who saw Labour as more restrictive had an average gain in the Labour vote of almost 5 percent across the categories of the immigration opinion scale, while those perceiving the Conservatives as more restrictive showed an average gain of almost 10 percent in the Conservative direction.[50] Overall issue voting, however, did not take place in 1966. The Conservatives had an average net gain of 1.9 percent on the four-point immigration opinion scale. This means an overall net effect for the Conservatives of 4.0 percent. Since the portion of the Butler-Stokes sample for 1966 analyzed here deviated 4.1 percent from the actual results, one would not be justified in concluding that even this observed total net effect in the sample existed in the population of England. Because voters perceived little difference between the parties on the immigration question, issue voting on immigration did not take place in the 1966 general election.

The 1970 results are considerably different. One complication is that the low number of respondents perceiving the Labour party as more restrictive does not allow one to draw statistically significant conclusions about this group. Hence we cannot say with confidence what role their opinions on immigration played in their voting behavior.[51] As expected, immigration opinion had no effect on the voting behavior of those who failed to perceive a difference bewteen the parties on this issue. For those who saw the Conservatives as the more restrictive party (and in 1970 this was 57 percent of the population), immigration was an important issue affecting their votes. A regression coefficient of .074 means that there was an average gain of almost 7.5 percent in Conservative voting as opinion toward immigrants increased in hostility among those who perceived the Conservatives as the more restrictive party. We must set aside our reservations about working with the small N of Labour perceivers if we wish to calculate what the net impact of the immigration issue was in 1970. When this is done, the Conservatives are found to have had an average net gain of 3.3 percent on the scale of immigration opinion in 1970. This means that the total net increment to the Conservatives on the basis of the immigration issue alone was 6.7 percent. The heightened perception in 1970 of the Conservatives as the party more likely to keep immigrants out resulted in this gain.

Issue voting on immigration in England took place only in 1970. Butler and Stokes would seem to be correct in contending that the potential of this issue was not realized in 1964 and 1966 because of the lack of generally perceived party differences on the question. Nevertheless, among those people who did perceive party differences on the issue, opinions did influence voting choice in these two elections. The Labour party gained votes among people who saw it as more restrictive, and the Conservatives were the beneficiary among people who perceived them as more restrictive. The murkiness of the aggregate mass perceptions enabled Labour to hold its vote loss in these years to a minimum.

The potential of this issue became manifest in 1970, as we have seen, largely because of the efforts of Enoch Powell, whose extremely restrictionist position on immigration, first enunciated in 1968, overwhelmed in the public mind such events as the limitation by the Labour government of the Kenyan Asians' entry into the United Kingdom. It has been well demonstrated that Powell did not create the high level of antipathy toward immigrants found in England.[52] What he did was to channel this hostility into votes for the Conservative party in 1970.

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[49] The overall R^2 in all three regressions shows that this particular set of variables accounts very well for the variance in voting behavior at the 1964 British general election when compared with previous efforts, even though it is not the purpose of the study to explain the vote, but only to determine whether knowing a person's opinion on immigration adds to the explanation. See Rose, "Britain: Simple Abstractions and Complex Realities," pp. 525–30; C. Neal Tate, "The Utility of Contextual Variables in Predicting Left-Right Voting Choice and Social Welfare Policy Preferences: Great Britain, Norway, the Netherlands, Germany" (paper delivered at Annual Meeting of the Southern Political Science Association, Atlanta, 1972).

[50] The overall explained variance again compares favorably with previous attempts at accounting for voting behavior in Britain, especially in the "Labour best" category.

[51] The total variance explained is much lower in 1970 than in the previous years. In the causal analysis, only variables directly or indirectly related to opinion on immigration were retained for the assessment of the impact of immigration opinion on voting behavior. Thus several of the variables contributing significantly to voting behavior, such as social class, are not included in the 1970 analysis. Since they did not affect immigration opinion, they could not account for any of the variation in the vote attributed to immigration opinion. The addition of social class to the variables in the full model in 1970 resulted in an R^2 for the three categories of perception of .35 (Labour perceivers), .23 (those seeing no difference), and .19 (Conservative perceivers).

Discussion

Since the foregoing conclusions on the impact of the immigration issue on electoral behavior touch upon a point of great political and emotional controversy in Britain, it is well to specify the precise nature and limitation of these conclusions and to deal with some of the objections which can be raised against them. First, we do not argue that the Conservatives "won" the general election of 1970 because of the immigration issue alone.[53] Issues, events, personalities, long-term partisan loyalties, and the demographic makeup of the voting population all affect the outcome of elections. While the vote is no longer quite the "great eleusinian mystery of the democratic state," there is still mystery enough there waiting to be unraveled.[54] One can assume that a particular issue was crucial to the outcome only if all other factors did not change from the immediately previous election. The other factors, however, are not static, but dynamic. Even the electorate itself is not the same from one election to the next. What one can say is that particular issues, events, personalities, partisan commitments, and demographic factors result in an advantage for one party or the other at an election. Labour, for instance, has a considerable advantage in any election in that 70 percent of the population of Britain is working-class, a group which normally splits its votes 70–30 in favor of the party of the working class. Labour does not win every election because its advantage on this factor and others is sometimes overwhelmed by disadvantages on other factors.

The foregoing analysis shows that the immigration issue constituted an advantage for the Conservatives in the election of 1970. Moreover, this issue provided an advantage not attributable to the influence of the background variables included in the analysis. The Conservatives gained a net of 6.7 percent of the vote on the basis of the immigration issue alone. While this is a strong effect for any single issue to have, its relative contribution to the Conservative victory depends on the effect of other variables, and many of these are not included in this analysis.

Chief among these missing variables is the relative contribution of colored voters to the totals of the two parties. Part of the Conservative advantage among white voters in 1970 on the immigration issue could have been offset if, as has been argued, Powell frightened colored people into voting for the Labour party.[55] Exactly how much of a net advantage to Labour this movement constituted, however, it is impossible to test here. It would also be valuable, of course, to learn what net advantage accrued from the immigrant vote in 1964 and 1966.

Other major influences on electoral behavior also remain unaccounted for here; for example, the effects of all the other public policy issues in these elections has been untouched. It is possible that the demonstrated relationship between the immigration issue and voting behavior could be a product of people's views on other policy issues, singly or jointly. In order to allay these suspicions, it would be necessary to incorporate individual views on these other policy issues into the multivariate analysis. The impact of political personalities has also not been explored, except for Powell's influence on the immigration question. Long-term partisan images and their effects in particular elections comprise yet another variable which needs to be examined. We have analyzed individual socioeconomic attributes, social contexts, and political contexts only if they affected both immigration opinion and voting behavior. The focus throughout has been on the impact of the immigration issue on voting behavior. In none of these elections, however, can we explain voting behavior solely by reference to opinion on immigration.

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[52] Studlar, "British Public Opinion, Colour Issues, and Enoch Powell: A Longitudinal Analysis"; Dilip Hiro, *Black British, White British* (London: Eyre and Spottiswoode, 1971); Elinor B. Bacharach, " 'Powellism' and Political Culture: The Impact of Race Problems on British Politics" (M.A. thesis, University of Chicago, 1969); W. W. Daniel, *Racial Discrimination in England* (Harmondsworth, Middlesex: Penguin Books, 1968).

[53] An excellent illustration of the pitfalls of attributing large changes in election results to single factors is found in Aaron B. Wildavsky, "The Intelligent Citizen's Guide to the Abuses of Statistics: The Kennedy Document and the Catholic Vote," in *The Quantitative Analysis of Social Problems,* ed. Edward R. Tufte (Reading, Mass.: Addison-Wesley, 1970), pp. 37–67.

[54] The phrase is that of R. B. McCallum as quoted in Richard Rose, *Politics in England* (Boston: Little, Brown, 1964), p. 3.

[55] Deakin and Bourne, "Powell, the Minorities, and the 1970 Election," 402–15. In general, ethnic minority voting patterns are a neglected research question, largely because of the difficulty of ascertaining trends in so small a portion of the population, even with large random samples. Three limited inquiries into this problem are Daniel Lawrence, *Black Migrants: White Natives* (Cambridge: Cambridge University Press, 1974), pp. 132–50; Community Relations Commission, Reference and Technical Services Division, *Participation of Ethnic Minorities in the October General Election, 1974;* and Muhammed Anwar, "Asian Participation in the 1974 Autumn Election," *New Community,* 4 (Autumn 1975), 376–83.

The objection might be raised that this analysis attributes too much effect to the immigration issue alone, not only in 1970 but also within the party perception categories in 1964 and 1966, by ignoring the factor of partisan identification. Several studies have found weak but significant relationships between partisan attachments and hostility toward immigrants in Britain.[56] The argument runs that many people acquire their notion of which party is more likely to keep immigrants out from their long-term partisan commitment. Perennial Labour supporters see their party in this role while their Conservative counterparts do likewise. Hence the opinion-vote connection is not as strong as it appears, since the interaction of perception and opinion depends on prior partisan attachment, which of course is also related to voting behavior. Butler and Stokes point out that partisan identification is highly correlated with voting behavior in Britain.[57] In the data currently under analysis, the correlations between partisan identification and vote were all above .94. Given such persistent results, Rose questions whether the concept of party identification apart from current voting choice is useful in studies of British electoral behavior.[58] Therefore, this objection can be reduced to the idea that people bring their perceptions of the parties and their opinions on immigration into line with their votes, rather than the reverse.

There is much evidence to indicate that the party a voter prefers on a particular issue is influenced by his or her general party preference.[59] Yet voters do not see their party as preferable on all issues. Parties cannot be all things to all people. Perception of which party is more restrictive, the key to the linkage of immigration opinion and vote, has always been weakly related to voting behavior because few saw any difference between the parties on immigration. Even in 1970, fully 35 percent of those surveyed saw no difference between the parties. Furthermore, perceptions of the parties changed radically between 1966 and 1970 while voting behavior changed much less and immigration opinion changed hardly at all. Undoubtedly some people's perceptions of the parties, as well as their opinions on immigration, are influenced by their commitment, long- or short-term, to a particular party. Nevertheless, the sharp increase in the percentage of those seeing the Conservatives as the more restrictive party in 1970 and the accompanying decrease in the percentage of people seeing Labour as more restrictive or seeing no difference between them indicates that perceptions are subject to influence by political events. It is highly unlikely that the increased opinion-vote relationship in 1970 indicates the influence of voting choice on perception of party differences. This could have affected a few individuals, but the roots of such a large change in perceptions, which in turn led to a stronger opinion-vote relationship, must lie elsewhere.

The most likely answer is that it was the saliency of Enoch Powell's anti-immigrant statements which led more people to see the Conservatives as the restrictive party. This large change in perceptions, in turn, favored the Conservative party electorally on this issue, as they had not been in previous years. This argument is supported by the only analysis of panel data on the subject, which shows that the perception of the Conservatives as the more restrictive party led some 1966 Labour voters to support the Conservatives in 1970.[60]

The most direct evidence, however, lies in the regression coefficients themselves. The *b* weight for the opinion-vote relationship among those perceiving the Conservative party as more likely to keep immigrants out was weaker in 1970 (.074) than in 1964 (.093) and 1966 (.098). Yet the total net effect coefficient for the whole sample in 1970 indicated that issue voting occurred then, but not in 1964 or 1966. The regression coefficient for Conservative perceivers was weaker in 1970 because more people who were both hostile toward immigrants and adamant Labour party voters had conceded that the Conservative party was more restrictive. Voting behavior for this group was detached from its perceptions on immigration policy, which decreased the relationship between opinion and vote among the Conservative perceivers. Since the Conservative perceivers constituted a much larger portion of the whole population in 1970, however, even this decreased relationship resulted in a net gain for the Conservative party on the immigration issue. The increased perception of the Conservative party as more restrictive allowed more people to link their immigration opinions with their votes. This change of perceptions, interacting with opinions, was enough to influence the vote in a Conservative direction.[61]

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[56] Richard T. Schaefer, "Party Affiliation and Prejudice in Britain," *New Community*, 2 (Summer 1973), 196–99; Richard T. Schaefer, "Correlates of Racial Prejudice," in *Sociological Theory and Survey Research*, ed. Timothy Legatt (Beverly Hills, Calif.: Sage Publications, 1974), pp. 237–64; E. J. B. Rose et al., *Colour and Citizenship* (London: Oxford University Press, 1969), pp. 557–59.

[57] Butler and Stokes, *Political Change in Britain*, 2nd ed., pp. 42–47.

[58] Rose, "Britain: Simple Abstractions and Complex Realities," pp. 496–97.

[59] Page and Brody give extended consideration to this phenomenon in "Comment: The Assessment of Policy Voting," 452–58.

[60] Butler and Stokes, *Political Change in Britain*, 2nd ed., p. 307.

In summary, the argument that voters adjust their perceptions of party differences on immigration to their partisan choice has some validity. Under such circumstances opinion on immigration only reinforces previous party choice. This cannot account completely, however, for the relationship between opinions on immigration and voting behavior in 1970. Immigration opinion had no discernible effect on voting behavior in 1964 and 1966 because the connection between parties and positions on the issue was not generally made. Once this connection was made, those opposing immigration, always a substantial majority of the electorate, flowed in large numbers into the Conservative ranks. The three conditions for issue voting were met in 1970, and opinion on immigration significantly affected voting choice.

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[61] Fox found considerable projection of the voters' immigration views onto their chosen party in the February, 1974 election. As has been argued previously, the Uganda Asians episode blurred distinctions between party policies. In such an ambiguous situation, it would be easier for voters to project their own immigration views onto their favored party than in 1970. Furthermore, in 1974 the Conservatives increased their vote over 1970 among those favorable toward immigrants and lost votes among those unfavorable; the Labour party situation was the reverse. See Fox, "Attitudes to Immigrants: A Comparison of Data from the 1970 and 1974 General Election Surveys," 175–78.

Measuring Politicians' Values: Administration and Assessment of a Ranking Technique in the British House of Commons*

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Despite their prominence in political affairs, values have rarely been studied through survey research. This article offers groundwork for quantitative investigations of politicians' values by describing the development, administration and assessment of a ranking technique in the British House of Commons. It uses tape-recorded interviews which suggest that values are intelligible components of politicians' belief systems and help identify difficulties in conceptualizing and measuring them. The ranking instrument employed to measure values demonstrates its adequacy by reproducing familiar cleavages between political camps, distinguishing ideological party factions and generating data related to themes MPs put forward when discussing institutions and policy problems.

From the politician's perspective, political conflict is about values, ends and means which protagonists believe desirable. To members of Parliament, and to journalists and historians as well, political man is an evaluating animal whose outlooks and battles are shaped by organizing principles. But such topics have rarely been pursued in quantitative research. This essay reports on efforts to measure politicians' values as part of a broader inquiry into their socialization and belief systems.

Compared with policy opinions and personality drives, values have surely been slighted. Survey research usually puts them aside in favor of asking politicians about current affairs, a strategy legitimized by relevance which wins agreeable interviews. Other studies more deliberately avoid values because they are regarded as rationalizations for deeper drives—the internal stimuli that motivate political behavior. Hobbes' power drives, and even Freud's Eros and Thanatos, have enjoyed greater attention than value analysis in contemporary elite studies.[1] An attractive feature of drive theories is their economy: by explaining a politician's actions with a few independent variables, they rescue us at a stroke from the labors of unraveling complicated value systems. Yet drives have not convincingly explained much variance in specific political beliefs and behavior; nor is it clear that they perform unique motivational functions unknown to the values which politicians themselves regard as important.[2] Thus, it seems appropriate to rehabilitate the values concept and prepare the ground for reconsidering relationships between political goals and actions.

This requires instruments which gather a broad range of value data and, at the same time, satisfy two practical constraints for interviewing national politicians: they must be acceptable to intelligent people of high status; and, they cannot consume too much of the limited time elites make available for academic interviews. The value-ranking technique passes these tests and generates data appropriate for analyzing representation, agenda setting, consensus and similar topics which involve relationships between values and other beliefs and behavior. The present paper proposes to develop methodological foundations for such investigations. After reviewing the values concept and the instrument's development, it describes difficulties in the form's administration and

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[1] Rational choice models often introduce power drives or office seeking as axioms: Joseph A. Schlesinger, *Ambition and Politics* (Chicago: Rand McNally, 1966); Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957). Freudian Theory has provided frameworks for studying prominent political leaders: Alexander L. George and Juliette L. George, *Woodrow Wilson and Colonel House* (New York: Dover, 1956).

[2] William J. McGuire, "The Nature of Attitudes and Attitude Change," in *Handbook of Social Psychology,* 2nd ed., ed. Gardner Lindzey and Elliot Aronson (Reading, Mass.: Addison-Wesley, 1969), III, pp. 147–48. See also Fred I. Greenstein, *Personality and Politics* (Chicago: Markham, 1969) and Robert E. Lane, *Political Thinking and Consciousness* (Chicago: Markham, 1969), pp. 20–22.

assesses its performance as a satisfactory measure of political ideals.

The Instrument's Development

Concepts and Sample. Attempts to measure values began with Thurstone's work in 1927 and today include many different scales used in psychology, sociology and anthropology.[3] Although political science lacks a comparable literature, there are several innovative explorations of politicians' values, most notably the International Study of Values in Politics project and an application of Lasswell's value inventory to content analyze party platforms.[4] Other studies have used values terminology in reporting data, though usually with less attention to definitions and theoretical discussion.[5]

The values concept covers a very wide range of phenomena. But among the diverse definitions two themes stand out: values are more general than other sorts of beliefs; and they shape the individual's evaluative experience.[6] Both are reflected in M. Brewster Smith's characterization of values as "highly generalized attitudes that define a person's orientation to life in terms of the things he deems most important."[7] Behind most definitions is the idea that people create order and meaning in their lives through basic principles which, when combined with assumptions about human nature, are sometimes referred to as a personal philosophy.[8] British politicians often express the core of their philosophies in a few compact images or personal anecdotes. For example,

> I remember quite distinctly: my governess asked me to paint certain historical characters and I instantly and instinctively disliked Oliver Cromwell and was instantly attracted by Charles I. And those feelings are still with me! Quite irrational, but later on, of course, backed up by argument.

Most politicians can carry on with the subject for hours; some write books about it. Obviously, the complexity and nuance of their views cannot be captured in standardized forms requiring all of 15 minutes to complete. Yet survey instruments can, I believe, identify satisfactorily some of their more important values. At the outset, we must be clear about what to look for, an undertaking complicated by the fact that value definitions come in so many different varieties. The distinctions which have substantial measurement consequences concern goals, awareness and generality.

Lasswell and Kaplan's oft-cited definition portrays values as goals, such as wealth or security, which individuals aim at and act to promote.[9] These can be either immediate *wants* or *ideals:* where affect is of primary interest, any want may count as a goal; however, if evaluation is emphasized, we seek goals which respondents regard as good or as ideals.[10]

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[3] L. L. Thurstone, "The Method of Paired Comparisons for Social Values," *Journal of Abnormal and Social Psychology,* 21 (1927), 384–400. See also William F. Dukes, "Psychological Studies of Values," *Psychological Bulletin,* 52 (1955), 24–50; and Stephen M. Pittel and Gerald A. Mendelsohn, "Measurement of Moral Values: A Review and Critique," *Psychological Bulletin,* 66 (1966), 22–35.

[4] Philip E. Jacob and James J. Flink, with Hedvah L. Shuchman, "Values and Their Function in Decision-Making," *American Behavioral Scientist* Special Supplement 5 (May 1962); Philip E. Jacob, Henry Teune and Thomas Watts, "Values, Leadership and Development: A Four Nation Study," *Social Science Information,* 7 (April 1968), 49–92; Zvi J. Namenwirth and Harold D. Lasswell, *The Changing Language of American Values: A Computer Study of Selected Party Platforms,* Sage Professional Papers in Comparative Politics, Vol. 1 (Beverly Hills, Calif.: Sage, 1970). See also Robert D. Putnam, *The Beliefs of Politicians* (New Haven: Yale University Press, 1973).

[5] Examples include Erwin C. Hargrove's report on goals and ideals: "Values and Change: A Comparison of Young Elites in England and America," *Political Studies,* 17 (September 1969), 339–44; and M. George Zaninovich's measures which include evaluative criteria and assumptions about human nature: "Elites and Citizenry in Yugoslav Society: A Study of Value Differentiation," in *Comparative Communist Political Leadership,* ed. Carl Beck (New York: McKay, 1973), pp. 226–97.

[6] Robin M. Williams, "The Concept of Values," in *International Encyclopedia of the Social Sciences,* Vol. 16, ed. David L. Sills (New York: Macmillan and Free Press, 1968), pp. 283–84.

[7] M. Brewster Smith, "Personal Values as Determinants of a Political Attitude," *Journal of Psychology,* 28 (1949), 477.

[8] Theodore M. Newcomb, Ralph H. Turner, and Philip E. Converse, *Social Psychology* (New York: Holt, Rinehart and Winston, 1965), pp. 44–45; Clyde Kluckhohn and Others, "Values and Value-Orientations in the Theory of Action," in *Toward a General Theory of Action,* ed. Talcott Parsons and Edward Shils (Cambridge, Mass.: Harvard University Press, 1951), pp. 409–12.

[9] Harold D. Lasswell and Abraham Kaplan, *Power and Society* (New Haven: Yale University Press, 1950), p. 16.

[10] This seems to be the point of Clyde Kluckhohn's distinction between "The desired" and "The desirable": Kluckhohn, p. 394. In constructing measures of criteria people apply in their evaluations, some studies operationalize values not as goals but rather as assumptions about human nature (e.g., "Man is trustworthy and perfectible") which are believed to be influential determinants of behavior. See, Florence Rockwood Kluckhohn and Fred I. Strodtbeck, *Varia-*

Closely related to this distinction is the contrast between *implicit* values, inferred from an individual's actions, and *explicit* values which are ideals he is aware of and whose importance can be discussed with an interviewer.[11] Finally, most commentators introduce a *generality* criterion to distinguish values from other evaluative beliefs such as norms, attitudes and opinions; values are said to be "extremely inclusive goals," few in number and not bound to particular contexts.[12] The present inquiry will treat values as political ideals, abstract conceptions of ends and means that are explicitly held by individuals and serve as standards for interpreting and evaluating their political experience.

The research design sought a dense sample, a compromise between thinly spread random samples and saturation samples which include everyone in an organization.[13] Of the British House of Commons' 630 members, 83 percent (521) were interviewed, 84 percent of whom (438) returned value-ranking data. Reasons for refusing interviews were quite diverse so that the population response rate is closely approximated among ministers, Opposition front bench spokesmen and all other subgroups examined thus far. Respondents were contacted between January 1972 and July 1973 (the Conservative Heath government being in power throughout this period) by five staff members, one of whom was the principal investigator who conducted a quarter of the interviews and supervised the remainder. These meetings produced an average of an hour and a half of tape-recorded conversation each, during the course of which subjects completed several written forms, including the value-ranking instrument. The tape transcriptions help us understand how MPs interpreted the value form and, thereby, provide a basis for the analysis at hand.

Measuring Politicians' Values. The value-ranking exercise, adapted from Rokeach's work, asks subjects to rank order a total of 36 values presented in four separate lists of nine values each.[14] In constructing the instrument, one strategy is to select values which the investigator considers theoretically significant.[15] But, if we wish to map a population's belief system, as in the present study, it is necessary to incorporate values which subjects *themselves* think central to their concerns. Although such value lists should ideally be compiled through exploratory discussions with interviewees, this procedure is unsatisfactory for small organizations where the developed instrument is later applied to the same population.[16] Fortunately, much has been written, though not without ambiguities and contradictions, about British political culture and its politicians' values, often by politicians themselves. An inventory of values was constructed from ideals expressed in parliamentary debates, commentaries by parliamentary correspondents, recent political memoirs such as those by Lord Butler and Sir Harold Wilson and academic attempts at synthesis.[17] We strove to include value distinc-

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tions in Value Orientations (Evanston, Ill.: Row, Peterson, 1961), pp. 1–48.

[11] Kluckhohn, p. 397.

[12] Theodore M. Newcomb, Ralph H. Turner and Philip E. Converse, p. 45; Robin M. Williams, p. 284; William J. McGuire, pp. 151–52; and Milton Rokeach, *Beliefs, Attitudes and Values* (San Francisco: Jossey-Bass, 1968).

[13] James S. Coleman, "Relational Analysis: The Study of Social Organizations with Survey Methods," in *Complex Organizations*, ed. Amitai Etzioni (New York: Holt, 1961), pp. 441–52.

[14] Milton Rokeach, *Beliefs, Attitudes and Values*, and *The Nature of Human Values* (New York: Free Press, 1973).

[15] There have been several attempts to identify value categories relevant to all cultures: Rokeach, *Beliefs, Attitudes and Values;* Lasswell and Kaplan, *Power and Society;* and Kluckhohn and Strodtbeck, *Variations in Value Orientations.* This approach runs the risk of constructing items which are so diffuse that they encompass everything, but, by the same token, may be related to few other beliefs and behaviors in any given political culture or political system.

[16] Bogdan Denitch, "Elite Interviewing and Social Structure: An Example from Yugoslavia," *Public Opinion Quarterly,* 36 (Summer 1972), 149. In order to insure that most of the important values have been identified, a considerable number of members would have to be interviewed.

[17] The most useful scholarly commentaries were: Samuel H. Beer, *British Politics in the Collectivist Age* (New York: Knopf, 1965); Jean Blondel, *Voters, Parties and Leaders* (Baltimore: Penguin Books, 1963); John P. Mackintosh, *The British Cabinet,* 2nd ed. (New York: Praeger, 1964); Robert T. McKenzie, *British Political Parties,* 2nd ed. (New York: Praeger, 1964); Peter G. Richards, *Honourable Members* (London: Faber, 1959); S. E. Finer, H. B. Berrington and D. J. Bartholomew, *Backbench Opinion in the House of Commons* (Oxford, Pergamon Press, 1961); Richard Rose, *Politics in England* (Boston: Little Brown, 1964); Peter G. J. Pulzer, *Political Representation and Elections in Britain* (London: George Allen and Unwin, 1972); James B. Christoph, "Consensus and Cleavage in British Political Ideology," *American Political Science Review,* 59 (September 1965), 629–42; and Allen Potter, "Great Britain: Opposition with a Capital 'O,' " in *Political Opposition in Western Democracies,* ed. Robert A. Dahl (New Haven: Yale University Press, 1966), pp. 3–33.

tions which, though they might seem trivial to outsiders, were said to be important to politicians.

After eliminating overlapping items, we revised the list through student pretests which identified confusing labels and helped sharpen definitions. Next, British scholars specializing in the study of politics and Parliament reviewed the revised instrument, making further suggestions about overlaps, definitions and possible additions and deletions. Final changes were based upon tape recorded pre-test sessions with former members of Parliament who were not returned in the 1970 general election and could not therefore appear in the study's sample. They filled in and discussed the value ranking form's penultimate version and suggested how it might best be presented to members of the House. The finished product proved quite satisfactory; "Honesty" was the only value interviewed MPs mentioned as missing from the lists.

In developing his technique, Rokeach produced a write-in version and another method using printed gummed labels which respondents move about until satisfied by the resulting ranks.[18] Since it was impractical to confront MPs with gummed labels, the project's form requested written ranks, as illustrated in Figure 1. Values were grouped in lists of 9, rather than Rokeach's 18, because shorter lists seemed likely to yield more complete and reliable responses. After a brief introduction to the notion of values and ideals, we presented the form with this instruction: "It would help us a great deal if you could rank the ideals in each group in the order of their importance to you. For each list, could you write the number one alongside the ideal which is most important to you? Then write two alongside the ideal which is second most important to you, and so on. In each group, the ideal which is least important should be given the number nine." Respondents completed the value sheet in 6 to 20 minutes, a period very similar to the time taken by most of Rokeach's respondents.[19]

For interviewing national elites there are not many alternatives to the value-ranking technique: respondents' reactions made it quite plain that even this format, handled as politely and pleasantly as possible, presses the limits of acceptability. Its major source of legitimacy was that most values were common coin of members' own political conversation; arranging them in the order of their importance to the individual did not seem utterly absurd on the face of it. Less familiar items, Rokeach's "Salvation" and "Loving" for example, would have met disdain and resistance. Likewise, semantic differentials, or a series of items of the sort included in standard value scales (e.g., "Which do you expect ultimately will prove more important for mankind–mathematics or theology?") would be regarded by many as bald insults to their intelligence and status; they simply "are not on" for face-to-face interviews in the House.

One feasible alternative is contextual coding of tape-recorded conversational interviews.[20] This has the important advantage of utilizing dimensions of meaning which are missed altogether by ranking instruments or conventional scales. On the other hand, its potential for sensitive measurement must be weighed against difficulties caused by missing data and problems of reliability and validity.[21] But the two approaches need not be mutually exclusive: by combining a ranking instrument with contextual analysis of taped discussions we can learn something from both worlds. Thus, interview transcripts provide circumstantial evidence that MPs interpreted the ranking form much as intended and that it measured values in a mode which was meaningful to them. For example:

> Now, List B. Well, number one undoubtedly is "Socialism," no question, although it means more than public ownership. And, I would think that "Participatory Democracy" must come number two. . . . "Capitalism," competition, private ownership I would almost put last–I'm just trying to see if there's anything which I regard as even worse. . . .

The measurement technique is based on an assumption that individuals repeatedly give precedence to the same values over others in situations requiring choice; in other words, that they do possess and apply value hierarchies which can be roughly represented on the value form.[22] This assumption must remain open to further investigation; even should it prove basically sound, however, value hierarchies may be neither well-crystallized nor inflexible. The point was made by MPs who, while accepting the plausibility of a rank-order model, noted that the exercise imposed more structure than usually characterized their thinking about the

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[18] Rokeach, *Beliefs, Attitudes and Values* and *The Nature of Human Values.*

[19] Rokeach, *The Nature of Human Values,* p. 30.

[20] Robert D. Putnam, *The Beliefs of Politicians* (New Haven: Yale University Press, 1973).

[21] The benefits and costs are reviewed by Joel D. Aberbach, James D. Chesney and Bert A. Rockman, "Exploring Elite Political Attitudes: Some Methodological Lessons," *Political Methodology,* 2 (Winter 1975), 1–27. On the advantages of conversational interviewing see also, Lewis Anthony Dexter, *Elite and Specialized Interviewing* (Evanston, Ill.: Northwestern University Press, 1970).

[22] It is often suggested that values are arranged in hierarchies; that values nearest the top are given first priority in guiding behavior and take precedence over

List A

6	AUTHORITY the right to command
3	COMMUNITY harmonious social relations
7	ECONOMIC EQUALITY equal wealth for all
1	FREEDOM independence, free choice
4	FREE ENTERPRISE absence of government control
2	PROPERTY private ownership
5	SOCIAL EQUALITY equality in dignity and privileges
8	SOCIAL HIERARCHY integrated social strata
9	UNITY solidarity

List B

1	CAPITALISM competition, private ownership
7	FELLOWSHIP companionship among equals
6	MERITOCRACY advancement by merit
8	PARTICIPATORY DEMOCRACY maximum political participation
5	PRIVACY avoidance of intrusion and publicity
4	PUBLIC ORDER maintenance of law and order
9	SOCIALISM public ownership and equality
2	SOCIAL PROGRESS continuous social improvement
3	STRONG GOVERNMENT decisive government

List C

3	CAUTION prudence and circumspection
2	COMPASSION concern for human welfare
9	DEFERENCE respect for superior judgment
1	DUTY fulfillment of obligations
4	INTELLIGENCE intellect and awareness
5	LOYALTY adherence to persons and promises
6	PATRIOTISM devotion to country
8	SELF-DISCIPLINE self-control
7	SELF-RELIANCE self-help, individual enterprise

List D

3	CO-OPERATION teamwork
7	DISCIPLINE obedience to the rules
8	EFFICIENCY skill and effectiveness
1	EMPIRICAL APPROACH being guided by experience
9	GRADUALISM wariness of sweeping change
6	RATIONALISM a rigorously reasoned approach
5	SECURITY freedom from uncertainty about the future
4	SOCIAL PLANNING application of practical principles
2	STRONG LEADERSHIP initiative, direction

Figure 1. Example of a Completed Values Form

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those below them in the event of value conflict. Asahel D. Woodruff and Francis J. DiVesta, "The Relationship Between Values, Concepts and Attitudes," *Educational and Psychological Measurement,* 8 (Winter 1948), 646; Kluckhohn and Strodtbeck, *Variations in Value Orientations,* p. 30; and Rokeach, *Beliefs, Attitudes and Values.*

subject. In particular, they emphasized that: (a) the values form forced them to choose between pairs of values they had never before considered competitors; (b) people differ greatly in the range of situations to which they apply a given ordering; and (c) the stability of value hierarchies varies considerably among their colleagues in the House.

Although the exercise provoked discussion and doubt, on balance it generally seemed reasonable to most respondents. If we assume that individuals arrange their values in a rough order of importance, how might this come about? Pairs of values like "Capitalism" and "Socialism" may be learned together as political antipodes; others may be deduced from a few core beliefs: "I am a Socialist and therefore value 'Social Planning' "; while the majority probably become ordered through everyday situations requiring that one be set above another: "If I find that British troops are mistreating even a few people in Northern Ireland, I'm moved more by 'Compassion,' concern for human welfare, than I ever would be by any ideas of 'Patriotism.' " Experience with a series of such choices leads people to develop rules of thumb for making judgments and taking action; they learn to regard some values as more vital than others. The ranking instrument's effectiveness is enhanced by the fact that it presents them with yet another, albeit artificial, situation in which priorities must be assessed and choices made.

Within each list, the value items are printed alphabetically; but deciding which values shall be placed in which lists is more difficult. A convenient criterion is the *ends-means* distinction Rokeach uses to postulate two distinct and separate value systems: terminal (*ends*) and instrumental (*means*).[23] The interview tapes suggest serious operationalization problems: *ends-means* classifications are not uniform across individuals—one man's end is often another's means. Indeed, sensitivity to this fact is a common resource used by politicians to manipulate one another's behavior. The Opposition may seek to convince a minister, for instance, that the end his bill proposes to promote is really a means to other ends he himself believes undesirable. Moreover, it is not obvious that *ends* and *means* values are compartmentalized or held separately; MPs may be asked to support the side and give "Loyalty," a *means* value, priority over their *ends* values when these conflict with party policy. They often do so because they realize that without loyalty their party will be unable to sustain the government or achieve any ends at all.

Despite ambiguity about what constitutes an *end* and what a *means*, the range of opinions on the subject may be constrained within special populations. From this perspective, a former member of Parliament suggested, in project pre-tests, regrouping the initial value items into lists which he thought British politicians might generally regard as *ends* and *means*. This was done, lists A and B in Figure 1 representing *ends* and lists C and D *means*. Many MPs recognized the criterion by which values had been sorted, even noting anomalies in the *means* lists, such as "Patriotism" and "Security," which are frequently considered *ends*. In the same vein, Rokeach reports greater reliability for his terminal than for his instrumental values, indicating that respondents may care more about those he calls terminal—which is as it should be if they are understood as *ends* rather than *means*.[24] Similarly, MPs found it easier to rank values in *ends* lists A and B than in *means* lists C and D where distinctions seemed difficult to draw. In sum, there is reason to believe that many respondents interpreted A and B as *ends* and C and D as *means*, though these were certainly neither firm nor universally held classifications.

Considering value systems and *means-ends* distinctions in this way highlights the instrument's stark abstraction and lack of context. It has been argued that these characteristics allow the measure to function as a semi-projective test: unaware they are revealing something about themselves, subjects have little incentive to disguise their values.[25] Yet several respondents did, in fact, comment on the self-examination implications of what they were doing. Some requested copies of the sheets; and one insisted on repeating the process after a fortnight to assess the stability of his rankings. Still, even these interviewees had little reason to hide their values: they could certainly never be held to account for ranks assigned in such an abstruse academic exercise. Indeed, members of Parliament usually work harder at publicizing their values than disguising them; not doing so could be counterproductive and, in some circumstances, stigmatized as dishonest. While it would be misleading to claim that the task was semi-projective for these politicians, I do believe they tried their best to give us an accurate picture of their values within the instrument's limitations.

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[23] Rokeach, *The Nature of Human Values;* cf. the distinction between intrinsic and instrumental values in Lasswell and Kaplan, *Power and Society,* pp. 16–17.

[24] Rokeach, *The Nature of Human Values.* By contrast, Kluckholn argues that *means* values, including ways of acting, are regarded as just as important as ends: "Values and Value-Orientations in the Theory of Action," pp. 403–13.

[25] Rokeach, *Belief, Attitudes and Values,* p. 26. It could be claimed that the value ranking form taps implicit as well as explicit values. A more cautious assessment would be that, in addition to measuring highly salient values, the instrument directs the subject's attention to others which, though prevalent in his sub-culture, he himself does not usually discuss.

Administering the Instrument

Completions and Interpretations. Measuring values was the interview's most delicate task. Members were invariably courteous and helpful; most completed the ranking exercise without serious objection, seeking only to clarify instructions and comment on the instrument's validity and probable results. The rest were more reluctant: despite considerable discussion and attempts at persuasion, 16 percent refused to touch the form.[26] Some said they did not have time for what they surmised was a lengthy task, and a few objected to the exercise as an invasion of privacy. A larger minority declined on the basis of vigorous intellectual objections to the instrument's assumptions. Several of their reservations have already been stated; others will be considered later.

But the majority among those who refused did not seem motivated by any of these reasons. Members granted the interview as a kindness to a professor, proposing to help him with his research by discussing political matters, views of Parliament and careers. What they found in this particular exercise was a "test," administered by overly clever university people, which put members in the position of pupils rather than statesmen or expert informants: "What a load of jargon. It's like an intelligence test in an exam. I'm just reading my way through all the lists first to see which one's the easiest—I do that in exams too." In retrospect, it is quite understandable that the exercise, and indeed any printed attitude scale or checklist, might be interpreted as an imposition on their generosity: few middle-aged or older people relish the thought of being tested in this way.[27] Satisfactory rapport was quickly re-established as the interview passed on to other topics, but 16 percent of the value sheets remained blank.

Interview transcripts provide data relevant to understanding how respondents decode abstract values.[28] Often subjects did request the contexts and sentences which some commentators claim are essential for thinking about values; and, when cues were not forthcoming from interviewers, several subjects introduced their own for the taped record. By and large, however, MPs appear to have treated the items more as abstract symbols than might have been anticipated. Here is an example of a typical request for context: "The most important to me, as what?" Our standard answer was: "For you personally, as an MP." It is my impression that rankings would have been substantially the same regardless of the type of ambiguous reply we offered, so long as we stressed our interest in a "personal" response. Beyond the minimal context of abbreviated dictionary definitions printed beneath each value item, respondents were left to their own interpretations.

MPs are well aware that their value profiles differ: one wrote alongside his personal scores the different ranks he believed would be awarded by most of his party colleagues; another recorded his impressions of ranks which would be forthcoming from the other side. Part of these differences, they claimed, are attributable to the fact that value items do not mean the same thing to all respondents. Judging by the transcripts there is evidence for this view, though it is difficult to say how widely interpretations actually differ. Several cases are unmistakable: Tories who ranked "Socialism" ninth put forward definitions ranging from totalitarian controls to a cumbersome economic system; "Social Planning" received high marks from both left- and right-wing Labour members, one seeing it as a road to equality, the other as a tool for managing the welfare state. Although the interview transcripts can alert us to variations in connotative meaning, the value-ranking technique itself is ill-equipped to capture them. Nonetheless, most respondents have been rubbing elbows and exchanging ideas for several decades in the small world of British politics. This experience might be expected at least to narrow the range of interpretations for familiar values in their environment.

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[26] Social scientists' forms—whether checklists, ranking exercises or Likert scales—have never been the highlights of a tired politician's week. See, William H. Hunt, Wilder W. Crane and John C. Wahlke, "Interviewing Political Elites in Cross-Cultural Comparative Research," *American Journal of Sociology*, 70 (July 1964), 59–68.

[27] According to Dexter, *Elite and Specialized Interviewing*, p. 36, one of the few satisfactions interviews offer elite respondents is the opportunity to teach someone, to explain matters about which interviewers know little and interviewees have the wisdom of experience. The value-ranking exercise, in the eyes of some MPs, apparently reversed these anticipated roles. An extreme example of the interviewee-as-teacher role is described by Harriet Zuckerman whose Nobel laureates peppered her with a stream of competence tests throughout their interviews: "Interviewing an Ultra Elite," *Public Opinion Quarterly*, 36 (Summer 1972), 159–75.

Steps by which MPs filled in values sheets can also inform us about how the instrument was interpreted. Everyone proceeded one list at a time, generally following the A, B, C, D order. Within lists, MPs would read all nine items before assigning the number 1, re-read the remaining eight and write in the number 2 and so on. After assigning their top two or three ranks, many subjects skipped on to identify the list's values they disliked most, rating these 9, 8 and 7, respectively. The remaining values were then distributed among middle ranks. Ranking from the bottom as well as from the top has been observed in other studies and reinforces the point that the technique produces ordinal rather than interval data.[29] Clearly, intervals separating ranks for different people may vary widely, as may intervals between different ranks recorded by a single individual. While Conservatives, for instance, generally ranked "Socialism" low, some were not satisfied with a mere 9 but insisted on writing 99 instead; and a few resolutely refused to allow "Socialism" a place even on the lowliest rung of their hierarchy: "There are some of these I don't agree with at all. I don't even rate them as worthy of listing." In the same vein, while many respondents easily ranked *means* values in lists C and D, others demurred at grading what they found more or less agreeable: "Hm. It's rather like asking what's one's favourite sunshine." Since values are ranked in relation to one another within lists, the technique measures only their relative importance.

It has been pointed out that value-ranking instruments force rank orders despite the possibility that individuals may regard several values as equally worthy.[30] Interview tapes confirm that this is a difficulty. The following comment was not unusual:

> I tend to put some at the front and some lower down . . . but there are some here I find it very difficult to distinguish between; . . . they're concepts which shade into one another. . . . I'm sure (were I to do this again later) my ones and twos would be very similar. But those between number six and number four would be all jumbled up.

Unlike undergraduates, members of Parliament cannot readily be dragooned into accepting rules which exclude ties: approximately one out of four began by awarding several values the same rank. For some subjects this was primarily a strategy to avoid tedious distinctions and hasten return to the political discussion; with others it was plain the ties accurately reflected a considered judgment. Interviewers were instructed, therefore, to discourage equal ranking and persist until it was clear that members did not in fact prefer one value over another. When pressed, some MPs eventually assigned ranks which seemed to us to reflect meaningful though difficult distinctions; yet 16 percent stuck to their guns and insisted on recording two or more ties. On the basis of this experience, it appears necessary strongly to discourage ties, while remaining prepared to accept them where respondents seem genuinely to regard values as equivalent.

Order Effects and Social Desirability Effects. Although values were arranged alphabetically within lists, it is possible that order effects occurred such that items at the top improperly won higher marks than those at the bottom.[31] This possibility was investigated by using Spearman's rho to correlate alphabetical order with the median value rankings MPs assigned each list's values. Rho equals +1.0 when rankings are in perfect agreement, 0.0 when there is no relationship at all, and −1.0 in circumstances of perfect disagreement. Order effects would be reflected in positive correlations between the two sets of ranks. There is no evidence of order effects for lists A, B, or C, whose rho correlations are −0.33, −0.31, and +0.02, none of which attains statistical significance at the .05 level. List D, however, does produce a positive correlation of +0.60 ($p = .05$).

Although this may indicate a genuine order effect, an alternative account merits consideration: the alphabetical arrangement of values in list D might roughly match many members' preferences. If the result is primarily due to order effects, these should operate throughout the list, in both its upper and lower sections. Partitioning the items into the top five and lower four entries reduces rho to +0.40 (n.s.) in

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[28] Donald N. Larsen, "Rokeach in Linguistic Perspective," *Review of Religious Research,* 11 (Winter 1970), 146–48.

[29] Rokeach, *The Nature of Human Values,* p. 30.

[30] Richard L. Gorsuch, "Rokeach's Approach to Value Systems and Social Compassion," *Review of Religious Research,* 11 (Winter 1970), 139–43.

[31] Rokeach, *The Nature of Human Values,* pp. 40–41.

the upper portion and to –0.80 (n.s.) in the lower portion, which suggests that an order effect explanation is not fully convincing. Still, despite these results for the sample as a whole, I am confident that order effects were at work among some subjects who, in ranking list C and especially list D, showed signs of fatigue or boredom and a desire to return to more rewarding discussion as quickly as possible. This also reinforces the suspicion that reliabilities for lists C and D are lower than those for lists A and B.

Another possible source of error is the social desirability effect where respondents grade values to enhance their image in the interviewer's eyes. This has been examined by an experiment which first presented a value-ranking survey under standard instructions; later the instrument was readministered with new instructions to rank values in an order which would make subjects appear admirable to the experimenter.[32] Absence of correlation between the two sets of rankings suggests that social desirability effects do not occur under normal conditions.

Social desirability effects are less likely with members of Parliament than with samples drawn from the general public, a difference due to relative status between interviewee and interviewer. University students might be anxious to see that instructors think well of them; middle-class citizens may react similarly to well-dressed survey interviewers. But MPs unabashedly characterize themselves as one of the most egocentric groups of people in Britain: we rarely suspected respondents of seeking to rub up their image for our benefit. In today's House of Commons, academics are almost as numerous as constituents; they are treated with the same politeness and generosity shown other visitors, but not with deference. Among all the interviews done by the principal investigator, only one respondent appeared to rank a value in a way obviously affected by my presence. This was a country squire from the Tory party who, upon reaching "Intelligence" in list C, said peculiarly, " 'Intelligence,' of course, is nothing but a nuisance—no doubt it deserves a nine."[33]

Investigations of Reliability and Validity

Reliability and Unreliable Pragmatists. Reliability refers to consistency, to whether the values measure can produce the same results in repeated trials. Some pragmatists claimed they are committed to unreliability as a matter of principle and refused to pledge themselves to a stable set of values. This was the main intellectual objection put forward by MPs who declined to fill in the form.

Confident of not having as stable a value hierarchy as some of their more ideological colleagues, these MPs rejected the exercise as meaningless for themselves; assigning ranks, they believed, would have conveyed a misleading impression of their views. The argument has two versions, both reflecting a concern with the relationship between values and their contexts in particular situations. One holds that the respondent does have a rudimentary hierarchy of values, but that it is very sensitive to political events and changes a good deal, even from week to week. Another position is that true pragmatists will only choose among values in specific situations, so much so that they never develop anything like the ranking instrument's hierarchical value systems. A pure pragmatist, it was claimed, simply does not have value orderings that exist apart from specific situations or that can be applied uniformly to most cases:

> If, for instance, I was beating you up in my house, my right to "Privacy" would be less than the public's right to "Public Order." But, if you had policemen knocking on your door at every hour of the night, then you would say that "Privacy" was dominant over "Public Order." I cannot arrange these in any static order because I believe that the order would change depending on the conditions.

Although it is difficult, in practice, to be so abstemious about using the abstractions of everyday speech (many did apply values across situations during the conversational interview), their objection is informative: it suggests that some people have substantially less stable values than others, a consideration which should be taken into account in assessing a value instrument's reliability. It also suggests that the hierarchical value system assumption bears investigation regarding proportions of individuals to whom it applies in varying degrees.

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[32] Rokeach, *The Nature of Human Values*, pp. 40–41.

[33] Besides social desirability effects, the researcher's presence may have other unanticipated consequences. In most cases interviewers watched respondents rank the values and were available to answer questions, albeit they were prepared to use pre-arranged answers wherever possible and avoid giving direction and structure. Rokeach reports an experiment which found no difference between rankings carried out privately and others done with experimenters in the room (ibid., p. 306). Again, there is reason to believe that members of Parliament, compared with undergraduates or the general public, are less disposed to react to an academic interviewer by altering responses that might have been recorded privately.

Test-retest reliabilities with Rokeach's value survey are quite good.[34] Form D, the movable label version, yields reliabilities in the .70s over periods of 3 to 12 weeks; his Form E, which requests written ranks as does the instrument used in the present study, has less impressive but still satisfactory reliabilities ranging from .65 to .74. While reliability tests could not be administered in the House of Commons, there was, nevertheless, one volunteer: skeptical of suggestions that the instrument would produce similar results after a fortnight, a Labour MP offered to take a retest. Spearman rho correlations between his value sheets, done two weeks apart, were +1.0 for list A, +0.93 for list B, and +0.97 with both lists C and D. In short, the rankings were nearly identical. Not surprisingly, Rokeach found that different values have different reliabilities, probably due to variations in their salience or comprehensibility for respondents. Several items on the political ranking instrument were not universally well understood either. The most troublesome were "Unity" and "Social Hierarchy," labels which academic commentators attach to politician's values, but which, transcripts show, are regarded as rather odd and foreign terms.[35] In the same vein, Rokeach noted that values ranked in the middle of lists changed most from test to retest. Likewise, it was the impression of project staff that members were least confident about these middle rankings: "I'm sure (were I to do this again later) those between number 6 and number 4 would be all jumbled up."

Validity: Pre-Tests with British Students. Value scales usually employ a series of items to measure each value; the ranking technique uses only one item per value which functions by comparison with others. This format is the source of its plausibility and practicality for interviewing politicians and other elites who have little time and less tolerance for standard scales. Still, the technique lacks multiple indicators and must therefore be examined with circumspection to insure that it adequately assesses respondents' outlooks.

In developing a British version of the value instrument, we felt it was desirable to explore validations for British subjects by correlating their ranking data with scores on better-known scales.[36] Since few politicians are eager to serve social science in this capacity, validations were pursued with 61 undergraduates at Oxford and the London School of Economics who completed, under the supervision of project staff, both the value ranking form and Allport-Vernon-Lindzey value scales.

The Allport-Vernon-Lindzey Study of Values (A-V-L) characterizes people by their dominant interests which include political, social and economic types.[37] Combining responses to 120 statements, the A-V-L produces scores for each type and is one of the best-regarded and most frequently used value measures. Administered to thousands of subjects over several decades, it has demonstrated substantial test-retest reliability and external validity. Since it measures values defined as dominant interests rather than the ideals sought in ranking instruments, this should work against the proposed validation by depressing correlations between the two scales.

Among all possible relationships between value ranks and A-V-L political, social, and economic scores, 37 percent proved significant at the .05 level or better; Pearson product-moment correlations ranged between .21 and .51. This seems an impressive outcome, particularly since the value-ranking items were not constructed with a view to their relevance for A-V-L categories. Most importantly, nearly every relationship represents a meaningful result when interpreted in the British context. For example, Allport-Vernon-Lindzey define their political type as a man interested primarily in power; and, "Strong Leadership" and "Authority" are the outstanding value ranking correlates of these political scores. Likewise, A-V-L social types, characterized by a love of people, give top marks to "Cooperation," "Consultation" and "Community." They are also said to reject the outlooks of economic men, finding them cold and inhuman. Consistent with this expectation, social and economic types take up opposite positions in the ranking data: every value related to the scores of both displays a positive sign in one case and a negative sign in the other.

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[34] Rokeach, *Beliefs, Attitudes and Values,* p. 169; and *The Nature of Human Values,* pp. 30–34.

[35] For example, a Tory who was baffled by the "Social Hierarchy" label described its intended meaning very clearly in answering a later question: "I do not believe in a classless society. You've got to have a hierarchy. And the hierarchy I hope will be based on merit. But you can't get rid of the hierarchy or you have a scramble." This respondent appears to hold the value but did not recognize its label in the instrument.

[36] A series of studies, largely American, have demonstrated that ranked values produce substantial and meaningful contrasts among groups differentiated by age, occupation, education, race, income and religion. Similarly, values measured by Rokeach's instrument have been related to a variety of beliefs including attitudes toward the Vietnam War and civil rights demonstrations, and to behaviors such as church attendance and joining civil rights organizations. See, Rokeach, *Beliefs, Attitudes and Values* and *The Nature of Human Values.*

[37] G. Allport, P. Vernon and G. Lindzey, *Study of Values,* 3rd ed. (Boston: Houghton Mifflin, 1970).

The ranking instrument apparently measures values captured by a well-known scale, according to interpretable patterns. Its validity was further explored by correlating value ranks with policy opinions which values, as organizing principles, are expected to structure. Student subjects responded to items on curbing strikes, increasing pensions and extending nationalization;[38] these views were related to their values at the .05 level or better in 41 out of 108 cells, with correlations ranging from .21 to .72. Again, the relationships are quite comprehensible: those in favor of curbing strikes were well-disposed toward "Property"; support for increasing pensions was related to "Equality" ranks; and, pro-nationalization respondents were in "Socialism's" vanguard.

Thus far we have reviewed the value instrument's development and administration, as well as reliability and validity experiments using students and other members of the public. Now I should like to see whether or not it works for politicians.

Candidates, Members of Parliament and Content Analysis

Politicians mobilize in like-minded groups to pursue political goals. This section probes the value-ranking technique's validity with a straightforward criterion: its capacity to segregate politicians from opposing political camps. Does the instrument measure what are regarded as major value cleavages between members of different political parties? Does it capture values reflected in their verbal behavior—in discussions of institutions and public policy?

Tories and Socialists. Tories and Socialists disagree about the public interest; by applying different criteria they reach quite different conclusions. Their political battles are seen by many participants and voters alike as a clash between two sets of values, between two understandings of what is politically desirable and how it might best be achieved.[39] We wish to know whether the ranking technique is attuned to these value differences, or whether, as one MP put it, "Yes, well it's not much more than pin-pricking. There you are."

Before we compare the responses of Conservative and Labour MPs, it will be instructive to set each alongside those of their party's candidates who were defeated in the 1970 general election.[40] If the instrument is measur-

Table 1. Comparison of Value Ranking by Candidates and Members of Parliament*

	Percent Assigning Each Value High Ranks (1, 2 or 3)		
	Mean Difference Between Conservative Candidates and Conservative MPs (%)	Mean Difference Between Labour Candidates and Labour MPs (%)	Mean Difference Between Conservative MPs and Labour MPs (%)
Value List A	6.0	3.9	28.9
Value List B	7.2	4.2	34.4
Value List C	5.6	4.6	13.8
Value List D	6.0	4.6	26.0

*Compares groups by percentage of respondents who rank each value first, second or third. Entries for the lists (nine values per list) are mean differences between the groups being compared.

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[38] The items are: Do you think the government should have the authority to curb strikes, or should it not have this authority? Do you feel the government should spend more on pensions, spend less, or should spending for pensions remain about as it is now? There is a lot of talk about nationalizing industry. Alternative views range from nationalizing many more industries to denationalizing many of the industries now nationalized. What do you feel should be done?

[39] Beer, *British Politics in the Collectivist Age;* Potter, "Great Britain: Opposition with a Capital 'O.' "

[40] For project purposes unrelated to this validity analysis, candidates fighting losing causes, usually as a service to their party, were isolated from the population. Similar considerations also excluded former members of Parliament, thus reducing the sampling frame to candidates who had never been MPs and who came closest to entering Parliament in 1970. From their ranks was drawn a random sample of 120 individuals, 89 percent of whom were interviewed and filled in the same value form as did MPs.

ing political values satisfactorily, we should expect to find much closer resemblance between candidates and MPs from the same party than between MPs on different sides of the political fence.

A party's candidates and MPs can be compared by the percentage within each group which awarded a given value top marks, scoring it either first, second, or third in its list. Table 1 summarizes these comparisons. The first two columns report mean differences, for each value list, between candidates and MPs. These gaps are quite small: of all 72 individual value comparisons (36 Conservative and 36 Labour) only a half-dozen distinguish candidates and MPs by more than 10 percent. The similarity between each party's candidates and its members of Parliament is remarkably consistent: at the .05 level there are virtually no significant value differences between them. Even raising the significance level to a more generous .10 merely produces a total of 13 significant differences, and modest ones at that. Candidates are a faithful image of MPs from their own political camp.

By contrast, Conservative and Labour members of Parliament are poles apart. They refer to one another as "the other side," and so they are: unlike the family resemblance between candidates and MPs, every value comparison save one produces differences which are statistically significant, usually at the .001 level. The percentage gaps involved are summarized for each value list in the third column of Table 1. Individual values separating Tories and Socialists most sharply are the familiar ones found in journalistic accounts: "Social Equality," "Economic Equality," "Freedom," "Socialism," "Social Planning" and "Public Order."[41] For each, party differences in assigning the top three ranks exceeded 40 percent. Before these data were analyzed, predictions about relative party preferences for different values were derived from the commentaries already cited.[42] Despite considerable ambiguity and sometimes disagreement among sources, it was possible to single out one party as likely to be more favorable than the other toward 30 of the value items. Twenty-seven of those predictions were confirmed.

The Labour movement's goals, which define its socialist essence, are deeply concerned with equality; this is the value said to mark best the divide between Labour and Conservative members of Parliament.[43] That divide is very broad indeed; with "Social Equality" it was the largest in the analysis: while 93 percent of Labour MPs ranked this value first, second or third, only 37 percent of their Conservative colleagues did so. Tory ambivalence toward "Social Equality" was expressed this way:

> I don't like "Social Equality." . . . That's not being fair, I do like "Social Equality." I am a great believer that when the Lord made us he made us equal in his sight. But I believe each of us has different qualities.

During the age of democratic revolutions, equality and freedom were allies in a struggle for political rights. But more recently, issues have shifted to social and economic ground where Socialists push hard for equality, and freedom becomes increasingly identified with defending the status quo; contemporary conflicts often pit the one against the other. Many Socialists have a very high regard for freedom, particularly as it applies to conscience issues of censorship and morality. Yet, when forced to choose, they opt for "Social Equality" as relatively more desirable because, " 'Freedom,' free choice, well yes . . . but not the choices of a private capitalist society which I believe are unreal and conditioned." Table 2 compares Conservative and Labour MPs by their relative rankings for "Freedom" and "Social Equality."

Four out of five Tories in Table 2 prefer "Freedom" to "Social Equality." This proportion is reversed among Socialists who suspect that freedom's unintended consequences may harm the weak, a misgiving sustained by Tory tendencies to dress out the symbol in free enterprise and laissez-faire: "Freedom of the individual is what I want; freedom to say what I want, freedom to start a business if I wanted, freedom to have property . . . they are all part of the same thing." Equality and freedom are closely associated with "Meritocracy," an ideal no longer as popular among Socialists as it once was. Despite the efforts of left-wing critics to drape this albatross round Labour MPs' shoulders, only 7 percent of them ranked it first, second, or third, as compared to 40 percent on the Tory side. Meritocracy became tarnished in socialist eyes through scuffles between opportunity for the underprivileged versus opportunity for the able.[44] Inequality based on merit is still inequality: "You know, I get worried about who decides what's merit," or, as another Labour member said: " 'Meritocracy,' advancement by merit, yes, although it's going to mean a very unequal society . . . I'm not terribly in favour."

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[41] See, for example, Paul Johnson, "What is a Socialist?" *New Statesman* (29 September 1972), 421–22; and Timothy Raison, "What is a Tory?" *New Statesman* (6 October 1972), 463–64.

[42] See footnote 17.

[43] T. B. Bottomore, *Elites and Society* (New York: Basic Books, 1964); Beer, *British Politics in the Collectivist Age*. Equality is also the key value which orders Americans along a left-right dimension: Rokeach, *The Nature of Human Values*.

My purpose is not to explicate Tory and Socialist belief systems in every respect, but rather to determine whether the value instrument measures meaningful distinctions between them. One additional example, then, should suffice. The road to "Social Equality" and "Economic Equality" is laid by "Social Planning," a *means* value on which Labour members led Conservatives by 53 percent. Allied with "Social Planning" are preferences for "Rationalism" and "Intelligence," ideals pushed to the background by Tories in favor of what they characterize as an "empirical approach."[45] These are all well-known value differences between Britian's political parties—the litmus tests used by many commentators to distinguish red from blue political interests. To this extent, the value-ranking technique produces an adequate performance: it generates data that tally with impressionistic accounts of political allegiance.

The instrument is also sensitive to factionalism within political parties. Conservative MPs were active in the right-wing Monday Club and in P.E.S.T. (Pressure for Economic and Social Toryism), a ginger group on the party's progressive flank. In the same way, social democrats, who are seen as Labour's moderates, can be contrasted with members of the Tribune Group, keepers of the socialist conscience. Taken together, these four factions represent successive steps on Westminster's ideological continuum extending from right to left. And their composite scores on nearly half the value items are distributed in exactly the same way. With "Public Order," for instance, the percentages of respondents ranking it first, second, or third are: Monday Club (71); P.E.S.T. (39); social democrats (12); and Tribune Group (03).

Content Analysis. Another approach to assessing the instrument's validity is to determine whether values measured by its paper-and-pencil technique are reflected in respondents' verbal behavior, in themes which they bring forward when discussing institutions and policy problems.

Those interviewed were asked to tell us what they regarded as the essential features of a real democracy. Encouraged to develop this in whatever direction seemed most important to them personally, they offered a synopsis of characteristics which often included participation and freedom. Two coders, whose intercoder reliability score was 77 percent on participation and 87 percent with freedom, examined the transcripts for the presence of these themes. The difficult cases were reviewed and either recoded by agreement or discraded from the analysis. Table 3 describes the relationship between individual propensities to mention each theme and value ranks for "Participatory Democracy" and "Freedom."

Many MPs are wary of participatory democracy's crowded corridors of power. Some introduced the topic only to criticize its populist implications; others stressed the need to listen to constituents' grievances, a theme not counted as participation for present purposes. But those who did mention participation in a favorable light were, by and large, the same people who graded "Participatory Democracy" high on the value instrument: two-thirds of them placed this ideal among the top three in its list, in contrast to only 30 percent of their colleagues (Kendall's Q = .66). They talked

Table 2. Relative Ranks for Freedom and Social Equality by Party Membership

Relative Value Ranks	Party Membership Conservative MPs (N = 209) (%)	 Labour MPs (N = 213) (%)
Percent ranking "Freedom" over "Social Equality"	85	21
Percent ranking "Social Equality" over "Freedom"	15	79
	100	100

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[44] C. A. R. Crosland, *The Future of Socialism*, rev. ed. (New York: Schocken Books, 1963).

[45] The conservative perspective is: "I think God in his infinite wisdom gave it to no man to know how to solve every problem . . . different groups of people warring with one another over different ideas is a much more efficient system than somebody who plans from above and says, 'Well boys, here are the tablets; now you can do it this way.' "

about the desirability of greater participation in local constituencies and communities, in national politics and, moving left across the political spectrum, in industry and other institutions as well: "And I want massive participation in democratic institutions. I think our schools should involve the children and their parents; factories should involve the workers and the technicians. I want to democratize all institutions and make them what I call publicly accountable." Similarly, 73 percent of the respondents who mentioned freedom as an essential feature of democracy had awarded "Freedom" top ranks, while only 58 percent of those to whom it was less salient did likewise (Kendall's Q = .32). Reference was often made to freedom of religion, freedom of political activity and, above all, freedom of speech:

> Government by consent, that's democracy. Now that to me is very valuable largely because of its by-products and the most important of its by-products being freedom of speech, freedom to write, freedom to argue. Democracy to me is great because it generates freedom.

Members of Parliament also spoke at considerable length about unemployment, a topic we introduced by asking them how critical a problem unemployment actually is. Almost everyone said unemployment was undesirable, but some saw it exclusively as an electoral issue: "Well, party-politically it is crucial. Second to prices, and very nearly equal top, unemployment is a factor which does affect elections, will affect the next election." Others analyzed it abstractly as a waste of resources or consequence of automation. Still others found it difficult to think beyond "the layabouts and unemployables in my constituency." Coders searched for something else, a compassion theme which referred to the unemployed's humiliation and suffering; these themes were identified with an inter-coder reliability of 74 percent. For example: "As one who knows what unemployment is, and I saw plenty of it in the thirties, I know how it can rot a man's will to even want to live. He loses his self-respect and everything." Table 3 shows that respondents who introduced compassion themes were likely to have ranked "Compassion" in first place, considerably more so than members who did not discuss the human costs involved (Kendall's Q = .55).

Table 3. Rank for Participatory Democracy, Freedom, and Compassion by Discussion of Value Themes

	Favorable Mention of Participation Theme	
Value Rank for "Participatory Democracy"	Participation Theme Present (N = 145)	Participation Theme Not Present (N = 230)
High (1–3)	67%	29%
Low (4–9)	33	71
	100	100

	Mention of Freedom Theme	
Value Rank for "Freedom"	Freedom Theme Present (N = 152)	Freedom Theme Not Present (N = 223)
High (1–3)	73%	58%
Low (4–9)	27	42
	100	100

	Mention of Compassion Theme	
Value Rank for "Compassion"*	Compassion Theme Present (N = 205)	Compassion Theme Not Present (N = 202)
High (1)	63%	30%
Low (2–9)	37	70
	100	100

*Since half the respondents scored "Compassion" number one in its list, this rank is set out by itself as the high score.

Summary and Conclusions

This inquiry has examined values as intelligible components of politicians' belief systems and has explored the extent to which they can be adequately studied through quantitative techniques. Thus far, evidence is encouraging on both points. The ranking instrument seems an efficient tool for describing values and analyzing their role in guiding political attitudes and behavior.

Tape recordings indicate that MPs interpreted the exercise much as intended and illuminate some of the structure and nuance in their value systems. Likewise, the transcripts identify difficulties with assumptions upon which the measure is based and also suggest improvements in its administration. Reducing the number of value items, for instance, would help minimize fatigue and dissatisfaction with the technique; two lists rather than four should, I believe, prove considerably easier to administer.

Ranked values did not appear to suffer unduly from social desirability or order effects. And Rokeach's versions of the value survey have established unusually strong test-retest reliabilities over periods of up to 12 weeks. Validity investigations, pursued through pretests with British university students, produced interpretable relationships between value-ranking data and the Allport-Vernon-Lindzey Study of Values as well as subjects' policy opinions. These results were followed up by further validity assessments which found that value data tapped familiar cleavages between politicians from different political camps and captured more subtle conflict dimensions by distinguishing ideological factions on the right and left of each parliamentary party. Finally, values measured by the instrument's paper-and-pencil technique were successfully related to verbal behavior in themes respondents put forward when discussing democratic institutions and public policy.

A remarkable tool—but of what use is it? Values have received such little notice from survey research that one wonders what might be done with them even if a satisfactory measure is available. Many psychologists and sociologists who have studied values suggest that they constitute the core of an individual's belief system, that values are basic principles which people use to guide their beliefs and behavior. Thus, value data should help explain differences in politicians' policy preferences, norms and perceptions of organizations and situations; they should also help explain behavior such as cross-voting, role priorities or participation in factions which attempt to influence party policy. Although values are sometimes suspected of being rationalizations for underlying drives, it is surely obvious that politicians can and do curb their impulse satisfaction in order to pursue long-range goals. There are, in fact, no convincing reasons to belittle values as symbolic flags with little relevance for behavior. They deserve serious investigation as significant components of political belief systems.

Workers' Councils and Political Stratification: The Yugoslav Experience

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Many students of citizen participation claim that one solution to the problem of political inequality in liberal democracies lies in the establishment of direct participatory channels in decentralized socioeconomic and political institutions similar to those found in Yugoslavia. Others argue that the availability of participatory channels in the workplace leads to the domination of these channels by a technocratic elite. Still other students of participation in Yugoslavia claim that participatory channels are dominated by the political elite, the League of Communists.

In this paper, we examine this set of conflicting hypotheses by using data which come from an extensive survey of participatory activities in four Yugoslav republics. Our findings are consistent with the interpretation that workers' councils open channels for a more technocratically oriented participation. When it comes to other kinds of activity, affiliation with the League is more important than socioeconomic or professional status in determining who participates. But because League members come disproportionately from upper-status groups, there is not a marked difference in the extent to which membership in workers' councils and participation in other kinds of activity are biased in favor of the advantaged segments of Yugoslav society. In each case, but for different reasons, it is the upper-status citizen who is likely to be active.

Yugoslav workers' councils are of great interest to the student of political participation. They provide direct participatory opportunities within small and proximate units. They give citizens a chance to take part in decisions about an aspect of their lives which they understand well and to which they devote a great deal of time. They give citizens a fuller opportunity to control decisions which affect their everyday life. At least, so argue advocates of participatory democracy and the Yugoslavs.[1]

But there are a number of questions that one can raise about workers' councils in Yugoslavia before they can be considered to be a model for participatory institutions elsewhere. Some of these include: Do workers' councils exercise real authority? Do they perhaps limit the extent of actual workers' control by co-opting workers into a management role? Are the processes within workers' councils democratic? And, how representative of the working population are the councils?

We shall concentrate on the last question.[2] It is an important question for understanding the effect of the councils on the Yugoslav participatory system as well as for understanding the potential impact of such institutions in other systems. Workers' councils represent new opportunities to participate in a setting where formal participatory opportunities would otherwise be closed to workers. We raise two sets of questions: 1. Do those who participate in workers' councils also participate in other aspects of Yugoslav political life or are they otherwise non-participant citizens made into participants through the opportunities provided by the councils? In other words, do those who dominate other aspects of Yugoslav political life dominate the workers' councils? 2. Are the participants in workers' councils drawn from all levels of the work force, or are workers' councils dominated by a technocratic or managerial elite? In other words, do those who ordinarily dominate the enterprise dominate the workers' councils?

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[1] See, for example, Paul Blumberg, *Industrial Democracy: The Sociology of Participation* (New York: Schocken, 1968); Robert A. Dahl, *After the Revolution* (New Haven: Yale University Press, 1970); G. David Garson and John Case, eds., *Workers' Control* (New York: Vintage Books, 1973), pp. 268–321; Carole Pateman, *Participation and Democratic Theory* (Cambridge: Cambridge University Press, 1970). See also Bogdan Denitch, "Notes on the Relevance of Yugoslav Self-Management," *Politics and Society*, 3 (Summer 1973), 473–91. For official views and discussion of Yugoslav self-management, see *Socialist Thought and Practice*, a journal published in Belgrade.

[2] Numerous articles and books deal with the first three questions. See the bibliography in Goldie Shabad, "Participatory Democracy: The Case of Yugoslavia" (Ph.D. dissertation, University of Chicago, 1976).

Political and Social Stratification

We deal with the question of workers' councils from the point of view of a more general analysis of the relationship between political and socioeconomic stratification. That general analysis is spelled out more fully elsewhere,[3] but brief reference to it will be useful. By *political stratification* we refer to differences in political activity and influence among citizens; by *socioeconomic stratification* we refer to differences in such social and economic advantages as income, material possessions, and education. Research in a number of contexts supports the generalization that, all else being equal, those citizens who are most advantaged in social and economic terms will be more active in politics. The reason for this is simply that such social and economic advantages and the socialization experiences in the family, school and workplace associated with an advantaged background provide the resources (material and intellectual) and the motivation (a greater concern for political matters, a sense of efficacy, the norm that one ought to participate) that make it likely that the "haves" will take greater advantage of participatory opportunities than will the "have-nots."

There are, however, a number of ways in which the tendency for the "haves" to take greater advantage of participatory opportunities can be overcome. Most of these alternative "paths to participation" involve individuals' affiliations with groups and institutions. The resources and motivations that derive from socioeconomic advantages are held by the individual as an individual; they make it more likely that he or she will, *all else being equal,* step forward as a participant. What may not be equal, however, are the institutions that link the citizen and the polity. Insofar as there are some institutions, such as political parties or voluntary associations, that influence access to the channels of political activity, these institutions may modify the likelihood that citizens will be as active as their individual socioeconomic characteristics would predict. They do so by mobilizing those affiliated with the institutions to political activity or inhibiting the activity of the unaffiliated.

The impact of such institutional recruitment or mobilization to political activity on the relationship between socioeconomic status and participation is, however, uncertain. If those affiliated with the institution come from lower socioeconomic levels, institutional mobilization will tend to *reduce* the overrepresentation of the "haves" among the participants. But if the "haves" are more likely to have institutional affiliation, mobilization to participation through institutional channels will *increase* the disproportion of the "haves" in the politically active segment of the populace. Analysis of data in the United States, for instance, indicates that the way American parties and voluntary associations recruit citizens to political activity produces the latter effect.[4]

We can consider participation in workers' councils in this framework. In Yugoslavia, individual socioeconomic forces play a major role in determining who becomes an activist; as in other countries, those higher in socioeconomic status are more likely to be active. In addition, institutional recruitment plays a major role, largely through the League of Communists.[5] We want to analyze how these individual and institutional forces affect involvement in workers' councils.

Consider the relationship between participation in workers' councils and socioeconomic advantage. One might have contrary expectations as to the impact of the availability of such participatory mechanisms within the workplace on the representativeness of the participant portion of the Yugoslav public. Such availability might tend to reduce the overrepresentation of the "haves" in the participant population. Workers' councils provide new opportunities for activity which ought to encourage some citizens to participate who were not previously active and this, in turn, might increase the representation of those lower in socioeconomic terms. In addition, because workers' councils are found in almost all enterprises (as well as in sociocultural institutions such as schools, hospitals and welfare agencies), they should give greater participatory opportunities to production workers. And, insofar as there are egalitarian norms that guide, if not determine, who should participate in workers' councils, then these might further induce the more disadvantaged groups to take part in workers' council activity.

On the other hand, the availability of this alternative mode of participation might merely

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[3] See Sidney Verba and Norman H. Nie, *Participation in America: Political Democracy and Social Equality* (New York: Harper & Row, 1972), Chs. 1 and 8; and Sidney Verba, Norman H. Nie and Jae-on Kim, *Participation and Political Equality: A Seven Nation Comparison* (New York: Cambridge University Press, forthcoming).

[4] Verba and Nie, Chs. 11 and 12.

[5] Verba, Nie and Kim, Chs. 4 and 9.

represent another opportunity to be seized by those already advantaged. This could happen in two ways. The chance to participate in workers' councils is open only to those who are employed in the socialist or public sector: i.e., in publicly owned industrial and agricultural enterprises and in sociocultural institutions. Those who are private farmers, private entrepreneurs, artisans, housewives, or unemployed have no such opportunity. Insofar as the "eligibles" are already among the more advantaged in Yugoslav society, participatory opportunities in the workplace will increase the disproportion of the "haves" in the activist segment of the population. One could argue that such a limitation of eligibility to those in the socialist sector may be unimportant, since those who are outside the socialist sector are barred only from taking part in decisions of enterprises or institutions with which they are not associated. But such a limitation may be significant for two reasons: insofar as the decisions of particular enterprises and sociocultural institutions have external effects on the economy and society as a whole, those who are ineligible for the functional self-management bodies may be barred from a sphere of decision making that does affect their lives. Furthermore, insofar as various forms of participation foster other forms–i.e., those who are active in workers' councils may learn to be active in other ways–the opportunity to take part in workers' self-management may give a participatory boost to those who have those opportunities that will make them more active and effective in other spheres as well.[6]

In addition, participatory opportunities in workers' councils might increase the participation gap between the "haves" and the "have-nots" simply because the "haves" will take greater advantage of the additional opportunity to be active. This process, seen in politics outside the workplace, may be accentuated within the workplace by the fact that decision making in the workplace tends to be technocratic in nature. Workers' self-management differs from other participatory channels in that it is not only a participatory system oriented towards promoting egalitarian decision making processes and associational democracy but in that it is also a functional-economic system oriented towards maximizing productivity and efficient performance. By their very nature, workers' councils are the focal point for many of the conflicting values and tendencies arising from Yugoslavia's simultaneous efforts to become both a modern industrial state and a socialist democratic society. Serving as a vehicle for the achievement of these two goals, workers' self-management is beset by profound conflicts between egalitarian and technocratic values, between democratic and meritocratic criteria for participation, and between tendencies towards "workers' solidarity" and tendencies towards functional and status differentiation based on expertise.[7] The push towards economic development signaled by the economic reforms of 1965 has tended not only to exacerbate these conflicts but also to shift the balance in favor of technocracy and its spokesmen, the "experts."[8] The more the acceptance of technocratic values and norms, the more will the highly trained and educated workers dominate workers' councils.

We also want to consider the relation between the availability of workers' councils and the important role the League plays in the process of recruitment to political activity in Yugoslavia. Workers' councils might provide an alternative channel to meaningful participation for those who are not affiliated with the League. This would be the case if the criteria for membership on such councils were specific to the enterprise or institutions (technical competence or popularity with fellow workers) and more important than the political criteria (commitment to the norms of socialism and the fulfillment of national goals) which ideally characterize the League member.[9] On the other hand, the existence of workers' councils might enhance the relative dominance of the League over channels of political activity if League membership were the key to membership in such councils. In the following sections of this paper we will present data on participation in workers' councils in Yugoslavia in order to assess the relationship between such activity and the role of socioeconomic and party recruitment forces.

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[6] Numerous writers have argued that opportunities to participate in the private sphere provide the training and skills to participate politically. See, for example, G. D. H. Cole, *Guild Socialism Restated* (London: Leonard Parsons, 1920); Alex Inkeles, "Participant Citizenship in Six Developing Nations," *The American Political Science Review*, 63 (December 1969), 1120–41; and Pateman, Chs. 4 and 5. For a discussion of these issues which uses the same data upon which this analysis is based, see Goldie Shabad.

[7] See W. N. Dunn, "Ideology and Organization in Socialist Yugoslavia; Modernization and the Obsolescence of Praxis," *Newsletter on Comparative Studies of Communism*, 5 (August 1972), 21–56; Frank Parkin, "Class Stratification in Socialist Societies," *British Journal of Sociology*, 20 (December 1969), 361; Miroslav Radovanovic, "Contemporary Yugoslav Society in Conflict with Itself," *International Journal of Sociology*, 2 (Winter 1972–73), 428; Sharon Zukin, *Beyond Marx and Tito: Theory and Practice in Yugoslav Socialism* (Cambridge: Cambridge University Press, 1975).

[8] For a discussion of the economic reforms and their political consequences, see Fred Warner Neal and Winston M. Fisk, "Yugoslavia Towards Market Socialism," *Problems of Communism*, 25 (November-December 1966), 28–37; and Dennison I. Russinow, "Laissez-Faire Socialism in Yugoslavia," *American Universities Field Staff Reports*, 14 (September 1967), 1–19.

[9] The criteria for membership in workers' councils and for the League of Communists need not be

The Data

The data we shall report come from a cross-national study of political participation in a number of nations. In Yugoslavia, a study was conducted of participatory activities in four republics—Croatia, Serbia, Slovenia, and Macedonia.[10] We shall draw on the data collected from these interviews. There are, of course, problems in conducting survey research in a society such as Yugoslavia—as there are, indeed, in most societies. It is our strong impression, however, that it is possible to get valid measurements of a variety of characteristics of the population as long as one does not ask about sensitive issues. Our questions are relatively straightforward and concern various kinds of participatory activities—all fully acceptable activities—as well as certain individual social and economic characteristics.

Participation in Workers' Councils: How Much? How Does it Relate to Other Activities? Before turning to the problem of stratification within workers' councils, let us consider some basic data on participation in Yugoslavia. Within the commune,[11] Yugoslav citizens have a multiplicity of channels, both indirect and direct, through which they may participate in political and socioeconomic affairs. Table 1 lists the political acts about which we asked as well as the proportion of our sample in the four Yugoslav republics who report engaging in each of the various kinds of political activity. The first eight activities are outside the self-government sphere and include such acts as voting, nomination of candidates for local office, attendance at citizens' meetings, joining with others in a collective action, and contacting government officials regarding personal or social problems. The last five activities in Table 1 represent membership in various kinds of self-governing bodies. These include membership in workers' councils (available only to those citizens employed in the socialist sector) as well as membership in more politically oriented self-government bodies such as local community councils, municipal councils and assemblies, and apartment house committees. The latter self-government bodies are responsible for certain decision-making processes concerning the affairs of various territorial units within the commune and, in contrast to membership on workers' councils, all citizens may participate in these councils.

The data demonstrate that participation in self-government activities is quite widespread in Yugoslavia, though it is by no means universal. Twenty percent of those in our sample report having been at one time a member of a workers' council and a small percentage report having been members of a variety of other self-management bodies. Interestingly, three times as many have been members of a workers' council as have been members of a local community council.

Insofar as elected membership in such a formal self-governing body such as a workers' council or a local community council represents a more continuing and probably more significant level of activity than those activities listed in the top section of Table 1, it is clear that the workers' council system does provide a significant alternative participatory opportunity for the Yugoslav citizenry. That one out of five of

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incompatible with each other. The role of "citizen-producer" and the importance of productive work are central to the League's ideology.

[10] These data were gathered as part of the Cross-National Program in Political Participation and Social Change, directed by Sidney Verba, Norman H. Nie and Jae-on Kim. The study was conducted in collaboration with Jan F. Triska of the Institute of Political Studies of Stanford University, and with Ana Barbic, Katja Boh, Dmitir Mircev, Pavle Novosel and Luba Stoic under the auspices of the Institute of Sociology and Philosophy in Ljubljana, Yugoslavia. The data were analyzed with the support of grant GS3155 of the U.S. National Science Foundation.

Here we will report only those findings based on the combined sample of all four republics. We are well aware that Yugoslavia is an extremely heterogeneous society and that there are major ethnic, cultural and economic differences among the four republics sampled in our survey. A previous analysis of these data, however, indicates that many of the overall patterns concerning the various kinds of participation that we find based on all four republics combined, were found in each of the four republics as well. See Sidney Verba and Goldie Shabad, "Workers' Councils and Political Stratification: The Yugoslav Experience" (paper presented at the 1975 Annual Meeting of the American Political Science Association), Appendix I.

[11] The commune is the basic socio-political unit in Yugoslavia. Each is approximately the size of a county in the United States.

our entire sample has at one time taken part in such a body is, we believe, impressive. If one considers only those who would be eligible to take part in such an activity—i.e., that 48 percent of the population employed in the socialist sector—one finds that 39 percent have taken part.

Participation in workers' councils is, therefore, fairly widespread. It is also a different type of activity, different from those others listed on Table 1. In a previous analysis of participation in Yugoslavia,[12] we sought to determine whether or not the various political activities were related so as to indicate the existence of alternative modes of participation comparable to those we discovered in other societies.[13] For our purposes here, the most important finding of this previous analysis of the modes of participation in Yugoslavia was that workers' self-management stood out as a separate mode of activity, different from both participation in indirect or "regular" political activity (the first eight acts listed in Table 1) and participation in more politically-oriented self-government bodies.

Workers' Councils as an Alternative Channel of Participation

We wish to compare the process by which citizens come to be active in workers' councils with the process by which they are mobilized to other kinds of activity. To do this, we distinguish among:

1) "Regular" political activity which includes participation in community action groups, membership on nominating committees, attendance at voters' meetings (electoral and non-electoral) and contacting some government official on a community problem. This covers a wide range of ways in which Yugoslavs can take part in political life outside of the self-management system and corresponds quite closely (with the exception of participation in the electoral process) to the kinds of activities found in other types of societies.

2) Political self-government activity which includes past or present membership in local community and municipal councils. It represents a parallel to workers' council activity in the area of local politics. Like workers' council membership, membership in a local self-governing body such as a local community council is direct, elective and for a fixed term.

3) Participation in workers' councils which is based on membership (past or present) in such a body.

Socioeconomic Status and Participation. We begin our analysis of the ways by which citizens come to be active in Yugoslavia by first examining the relationship between socioeco-

Table 1. Percent Engaging in Thirteen Acts of Political Participation[a]

Regular voters	82%
Worked with a local group in a large-scale community action	57
Engaged in electoral activity	45
Attended voters' meetings	45
Worked with a local group in a small-scale community action	45
Contacted an official about a personal problem	20
Contacted an official about a social problem	11
Member of an electoral nominating committee	9
Self-Government Activities	
Member of a workers' council	20
Member of a local community council	7
Member of an apartment or house committee	6
Member of one or more municipal councils	4
Member of other self-government bodies	1
N = 2995	

[a] In each case, a respondent was considered to be active in relation to a particular act if he reported ever having engaged in it.

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[12] Sidney Verba, Norman H. Nie, Ana Barbic, Galen Irwin, Henk Molleman and Goldie Shabad, "The Modes of Participation: Continuities in Research," *Comparative Political Studies*, 6 (July 1973), 235–50.

[13] Sidney Verba, Norman H. Nie and Jae-on Kim, *The Modes of Democratic Participation: A Cross-National Comparison*, Sage Professional Papers in Comparative Politics 01–013, 1971, pp. 15–44.

nomic status and participation. Table 2 presents data on the proportion of each of the three activist groups coming from the top third of a socioeconomic scale which is based on the individual's income and educational level. In Table 2 we present percentages based both on the population as a whole and on the socialist sector only (that segment of the population that is eligible to participate in workers' councils). Our data support other findings that the availability of workers' councils tends on the whole to provide an additional opportunity for those with upper socioeconomic characteristics to participate. Our data, furthermore, show that participation in workers' councils, of the three kinds of activity, is the one most biased in favor of the "haves" in Yugoslav society.

The greater bias of workers' council activity when using the whole population as the base is primarily due to two factors. First, only a part of the population is eligible to participate in such activity and that segment tends to be heavily weighted with people from the upper reaches of the socioeconomic scale. Fifty-five percent of the eligible segment comes from the top third of the scale, while only ten percent come from the bottom third. In contrast, 54 percent of the ineligible segment come from the bottom third of the socioeconomic scale and only 11 percent from the top third. Second, even among those eligible to participate, the selection process is such that the better-educated and more affluent citizen is more likely to become a member of a workers' council. Thus, when we consider the socialist sector only, we find that participation in workers' councils continues to be skewed in the direction of the "haves" in Yugoslav society.

No such bias occurs in regular political activity because it is more widespread throughout Yugoslav society and, thus, a more representative group of people engages in it. This difference is also the result of the particular kind of participation—functional and technocratic—entailed in membership in workers' councils, which distinguishes it from regular political activity. When we compare workers' council activity with participation in political self-government bodies, however, we find little difference in the proportions of workers' council members and political self-government activists who are upper-status citizens. Both types of activists come disproportionately from the "haves." That there is so little difference between the two activist groups in this respect, despite the fact that political self-government is even less widespread than membership in workers' councils, only serves to highlight the potency of socioeconomic factors coupled with the functional nature of workers' council activity in determining who becomes an activist in workers' self-management.[14]

Table 2. Proportion of Activists Types[a] Who are Upper Status Citizens

	From Whole Population	From Socialist Sector
Total	33%	33%
Regular political activists[b]	23	26
Political self-government activists[c]	41	47
Workers' council members[d]	66	41

[a] We present only the proportion of activist types who are upper status citizens since that for lower status citizens is the mirror image of the data presented above. See Verba and Shabad, p. 30.

[b] These activists engage in varying amounts of regular political activity, i.e., attending voters' meetings, participating in communal actions, contacting governmental officials—but do not participate in either of the self-government bodies.

[c] These self-government activists also engage in regular political activity but do not participate in workers' self-management.

[d] This group also engages in regular political activity but does not participate in political self-government councils.

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[14] Analysis of the occupational composition of the three activist types show a similar pattern of greater upper-status bias in relation to workers' self-management activity than in relation to other kinds of activity. Skilled workers are more likely to be workers' council members than unskilled workers; and highly educated white collar employees are more likely to be workers' council activists than more poorly educated white collar employees. A single exception to the greater bias of workers' self-management in comparison with other kinds of activity is the greater overrepresentation of skilled blue-collar workers among workers' council members than among other activist types. In this sense, workers' councils do provide a participatory opportunity for a group that would otherwise be relatively inactive in politics. See Verba and Shabad, pp. 32–36; and Shabad, Ch. 5.

Data gathered over two decades indicate that the bias of workers' council activity towards the better educated white collar employee is increasing dramatically, especially since the introduction of the 1965 economic reforms. See Shabad, Ch. 8.

Organizational Affiliation and Participation. In addition to socioeconomic status, organizational affiliation also plays a significant role in mobilizing citizens into the political process. One of the major channels of Yugoslav political activity is membership in the League of Communists. League members are expected to be leading participants who set an example for the rest of society. One would expect therefore, in part because workers' councils play such a leading role in Yugoslav socioeconomic life, that members of workers' councils will come disproportionately from the League's ranks. Though League membership is not a formal requisite for workers' council membership, the kind of person who would be a League member is also likely to take an active role in workers' councils. (It may also work the other way around, however. Very often politically active citizens are recruited into the Party, or they may join the Party because it is an additional and important vehicle for upward mobility in Yugoslav society.)[15] On the other hand, workers' councils may provide an alternative participatory channel for those who are not affiliated with the League. This would be so if technocratic and professional criteria are more important for participation in the workplace than political or ideological criteria.

Table 3 presents the proportions of the three activist groups who are members of the League of Communists. Column 1 uses the whole population as the base; column 2 considers only the socialist sector. The data in Table 3 illustrate the importance of League membership for both types of self-government activity. Self-government activist types are more likely to be League members than are those who engage solely in regular political activity.[16] Close to one-third of workers' council members and political self-government activists are League members compared with 7 percent of regular political activists. But while League membership enhances the likelihood that one will be a workers' council participant, party affiliation plays a more important role in relation to political self-government involvement than in relation to participation in workers' self-management. This is even more evident when we consider the data for the socialist sector. Within the eligible population, political self-government activity depends much more heavily on League membership than does workers' self-management. Forty-four percent of political self-government activists are League members compared with 26 percent of workers' council activists.

The data on League affiliation differ from those for socioeconomic status. They suggest that League affiliation, though important for both kinds of self-government activity, is less important in relation to workers' councils than in relation to political self-government. The socioeconomic data tell the opposite story. Membership in workers' councils is more closely related to socioeconomic status than are other activities. This contrast tells us a great deal about the different recruitment processes to these various kinds of activities.

Socioeconomic Status and Organizational Affiliation: Their Interaction. We can get a more complete picture by looking at the way in which League membership interrelates with socioeconomic status in determining who becomes an activist in Yugoslav society. We can do this by examining how socioeconomic status and League membership affect what we have called regular political activity.

Figure 1 plots the relationship between socioeconomic level (on the horizontal axis) and the level of regular political activity (on the vertical axis). The latter is measured by a scale summarizing the individual's level of regular political activity where the mean for the population is zero and the standard deviation of the scale is 100. Negative scores mean that the individual participates below the mean for the population as a whole. We plot this relationship between socioeconomic level and regular political activity for the whole population as well as for three population groups: members of the League of Communists, members of the Socialist Alliance[17] (who are not also members of the League), and those unaffiliated with either.

When we look at the population as a whole, we see a moderately sharp relationship between socioeconomic status and political activity: the higher the socioeconomic level, the more active one is. However, when we look at the various population groups differentiated by their affiliation or lack of affiliation with sociopolitical institutions, we find that institutional affiliation plays a much bigger role than socioeconomic status in determining who becomes a regular political activist. All League members, no matter what their socioeconomic level, are high in regular political activity. Socialist Alliance members are moderate regular political activists, but, again, socioeconomic status plays no particular role. And those affiliated with neither the League nor the Alliance are relatively inactive, no matter what their socioeconomic level. The data suggest that institutional affiliation (especially with the League) is both a necessary and sufficient condition for high levels of regular political activity in Yugoslavia.[18] If you are not a member of the League you do not engage in high levels of activity no matter what your socioeconomic status. If you are affiliated with neither of the two your political activity level is low no matter what your socioeconomic status. Thus, these two institutions clearly represent major keys to active political life in Yugoslavia.

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[15] See Garson, p. 36; Parkin, 364.

[16] If we distinguish regular political activists according to their degree of activity, we find that within the eligible population, those who engage in high amounts of regular political activity are more likely to be League members than are workers' council members. See Verba and Shabad, pp. 39–46, for a more detailed analysis of the relationship between party affiliation and participation.

[17] The Socialist Alliance is a mass organization of over 8 million members. While League members as well as nonmembers belong to the Socialist Alliance, it accepts the basic premises of the League and its leadership come primarily from League members.

The data also make clear how League membership relates to socioeconomic status in affecting who engages in regular political activity in Yugoslavia. Note that the line relating socioeconomic level to political activity for the whole population slopes upward but is flat within the various groups defined by their institutional connections. The reason is that the presence or absence of League membership erases the effect of socioeconomic status. But League members come disproportionately from those high on the socioeconomic scale, while the unaffiliated come disproportionately from those low on the socioeconomic scale. This is shown in Table 4 which indicates the sharp overrepresentation of the "haves" among League members—71 percent of the League members come from the top one-third of the socioeconomic scale and only 6 percent from the bottom third.[19] This means that League affiliation plays a dual role in the relationship between socioeconomic level and political activity. On the one hand, because League members tend to be very active and unaffiliated citizens tend to be much less active (regardless of their socioeconomic status), League membership represents an institutional channel to regular political activity that bypasses the path to participation deriving from the individual's socioeconomic resources and motivations. But because League affiliation is so closely associated with socioeconomic level, the activist population comes disproportionately from the more affluent and better educated. On the whole, then, the League is the channel into political activity for the "haves" of Yugoslav society.

Table 3. Proportion of Activist Types Who are League Members

	From Whole Population	From Socialist Sector
Total	15%	26%
Regular political activists	7	15
Political self-government activists	30	44
Workers' Council members	26	26

Table 4. Socioeconomic Characteristics of League of Communists Members and Socialist Alliance Members

Socioeconomic Level	Unaffiliated	Alliance Members[a]	League Members[b]
Lowest 1/3	46%	25%	6%
Middle 1/3	34	39	23
Highest 1/3	20	36	71
	100	100	100

[a] But not League.

[b] Possibly also Alliance members.

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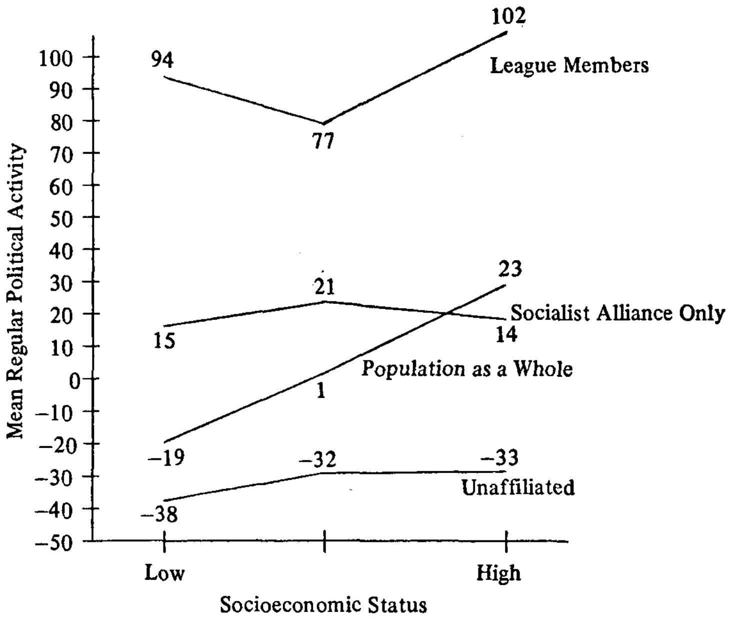
[18] This is not to say, however, that nonmembers do not participate. Their participation rate is, nevertheless, substantially lower than that of Alliance or League members. See Jan F. Triska and Ana Barbic, "Evaluating Citizen Performance on the Community Level: Does Party Affiliation in Yugoslavia Make a Difference?" (unpublished paper, 1975).

[19] Looking at the same data in a somewhat different way, 2 percent of those low on our socio-

Figure 2 which shows the interrelationship among socioeconomic status, party affiliation and participation in political self-government, parallels the pattern found in relation to regular political activity. Figure 2 takes the same form as Figure 1, but the vertical axis contains a measure of activity in political self-government–i.e., the proportion of a group active in such bodies.[20] As with regular political activity, there is a fairly strong relationship between socioeconomic level and participation for the population as a whole: the higher one's socioeconomic level, the more likely one is to be active. Those high on the socioeconomic scale are four times as likely to be members of self-governing bodies in the political sphere than are those low on the scale. But like regular political activity, affiliation with a political institution, especially with the League, plays a much bigger role than does socioeconomic level. The data suggest once again that if one is affiliated with the League, one is much more likely to participate in political self-government councils, regardless of socioeconomic status. The League represents a major key to this kind of political activity as well.

Figure 3 contrasts the patterns found in relation to regular political activity and to political self-government activity with that found in relation to participation in workers' councils. It takes the same form as Figures 1 and 2, but the vertical axis now represents a measure of activity in workers' councils–i.e., the proportion of a group active in workers' councils.

Figure 3 does show that those affiliated with the League are more likely to be active in workers' councils no matter what their socioeconomic level. This is seen in the fact that the line for League members is well above that for the unaffiliated (as well as above that for



Regular Political Activity here is a standardized additive scale (electoral activity, communal activity, and contacting with a social referent–political self-government *not* included).

Figure 1. Regular Political Activity by Socioeconomic Status and Party Affiliation

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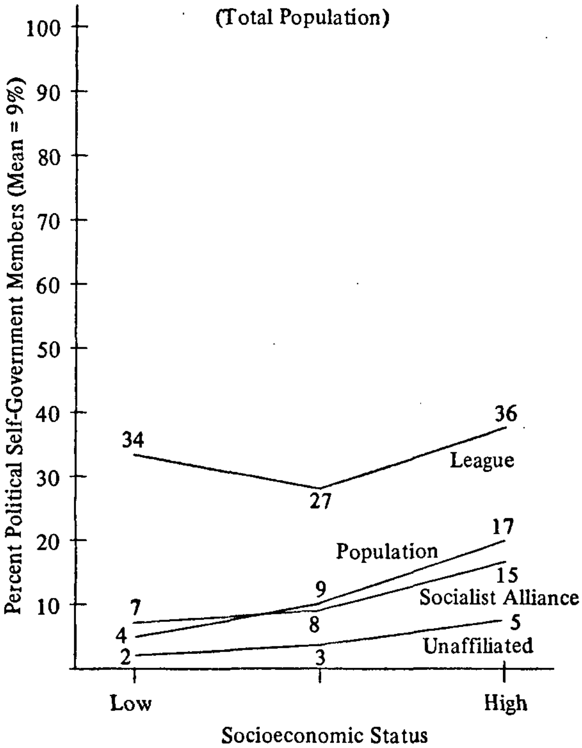
economic scale are members of the League, while 26 percent of those in the top third of the scale are League members.

[20] The variables on the vertical axis in Figure 1 and Figures 2, 3 and 4 are not equivalent in the absolute sense, but they do allow us to highlight the different ways in which the two forces–socioeconomic resources and institutional recruitment–interact in relation to regular political activity, political self-government and participation in workers' councils.

Alliance members) at each socioeconomic level. But in contrast with Figure 1 on regular political activity and Figure 2 on participation in political self-government, League affiliation is neither necessary nor sufficient for activity in workers' councils. Among those who are League members, among those who are Socialist Alliance members, and among those affiliated with neither institution, socioeconomic level still plays a major role (a fact reflected in the positive slope for each of the lines). In other words, the individual resources associated with socioeconomic level play a major role in determining who will become a workers' council member.

Some comparison of Figures 1 and 3 will make this clear. Consider the League members from the lowest third of the socioeconomic scale. When it comes to regular political activity, they participate about one standard deviation above the mean for the whole population and about as much as do those League members from the top of the socioeconomic hierarchy (Figure 1). For this group, affiliation with the League appears to be sufficient to overcome the low potential for political activity that one would expect from a group near the bottom of the socioeconomic scale. Compare this with the situation for the same group when it comes to membership in workers' councils (Figure 3). While a greater proportion of League members than non-League members at each socioeconomic level participate in workers' councils, there is nevertheless a 30-percentage-point difference in frequency of membership between those League members low on the socioeconomic scale and those high on the scale.

A similar comparison can be made of those whose institutional characteristics would be likely to lower their activity level, but whose individual social characteristics would be likely to raise that activity level. These are the high socioeconomic status citizens unaffiliated with either the League or the Alliance. As Figure 1 shows, they are quite inactive in relation to regular political activity; no more active indeed



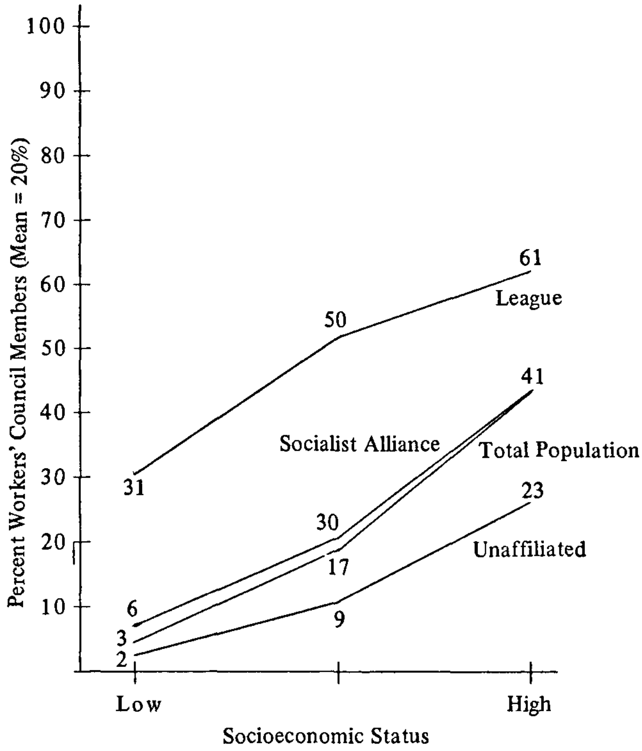
(Note: Socioeconomic division based on 1/3's of the total population.)

Figure 2. Percent Members of Political Self-Government Councils by Socioeconomic Level and Party Affiliation

than those unaffiliated citizens from the bottom third of the socioeconomic scale. Their individual resources appear to play little role in their level of regular political activity. The lack of League affiliation seems to explain their low political activity. Compare the situation with that found in Figure 3 in connection with participation in workers' councils. A much greater proportion of unaffiliated citizens from the upper socioeconomic level are active in workers' councils than are unaffiliated citizens low on the socioeconomic scale.

The contrast between Figures 1 and 3 (and the similar contrast between Figures 2 and 3) highlights the special nature of the recruitment process to workers' councils. As with other activists in Yugoslavia, affiliation with the League of Communists plays a major role in connection with membership in workers' councils, but it is not the sole criterion for such activity. The individual resources associated with higher socioeconomic level also play a role. Indeed, they appear to play the more important role—the difference between someone in and out of the League is, on the whole, not as great as the difference between those high and low on the socioeconomic scale when it comes to participation in workers' councils.

Figure 4 repeats the data for Figure 3, but for the population employed in the socialist sector. This eliminates any effects of the differential eligibility for participation in workers' councils across social groups. It presents a pattern quite similar to that in Figure 3 and would lead to no major modification of the overall conclusions reached from that Figure. The pattern found in Figure 4 reflects how workers' council members are recruited from the available pool of participants. League affiliation is important; the League member is more likely to be active in workers' councils. But an individual's socioeconomic characteristics over and above his institutional affiliation also play a major role. Further, it is also clear that socioeconomic status is more important for those outside of the League. For those in the League,



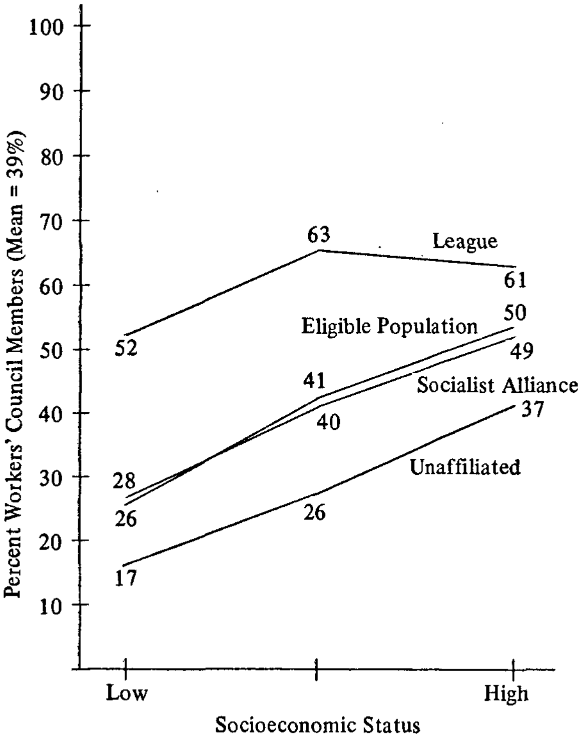
(Note: SES division based on 1/3's of the total population.)

Figure 3. Percent Workers' Council Members by Socioeconomic Level and Party Affiliation (Total Population)

there is only about a 10-percentage-point difference in frequency of membership in workers' councils between those low on the socioeconomic scale and those middle or high on the scale. For those not members of the League (i.e., those affiliated with the Alliance or unaffiliated), the gap in workers' council participation frequency is 20 percentage points between the low and high socioeconomic groups. League membership, this suggests, partially overrides the effects of socioeconomic level when it comes to participation in workers' councils for those who are members of the League. But nonmembers of the League are not locked out of this activity as they are in relation to regular political activity. The resources and motivation associated with higher socioeconomic status can lead to membership on workers' councils despite the lack of League affiliation.

One more comparison between Figures 2 and 3 makes this clear. Consider two opposite types of citizens: the lower-status League member and the upper-status nonmember. The former has the institutional affiliation needed for activity but not the socioeconomic resources. The latter has the socioeconomic resources but not the institutional connection. When it comes to participation in political self-government councils (Figure 2), the League member who is socioeconomically disadvantaged greatly outparticipates the opposite number high on socioeconomic resources but unaffiliated with the League. When it comes to workers' councils (Figure 3), the high-status nonmember of the League is more likely (if an Alliance member) or almost as likely (if totally unaffiliated) to belong to such a council.

The data in Figures 1 through 4 tell us a great deal about the kinds of individuals who



(Note: SES division based on 1/3's of the eligible population.)

Figure 4. Percent Workers' Council Members by Socioeconomic Level and Party Affiliation (Socialist Sector Only)

become members of workers' councils in contrast with those who become active in other aspects of the political process. The data also tell us how these individuals become involved in various kinds of activities. The main route to regular political activity and to participation in political self-government councils appears to be through the League of Communists. Socioeconomic resources play a role, but they do so by increasing the likelihood of affiliation with the League. Without that affiliation there is little activity. Those unaffiliated tend to be relatively inactive—no matter what their socioeconomic level. The fact that membership in the League eliminates differences in activity rates across various social levels does not mean, however, that the activist population is a representative cross-section of those social levels. Quite the contrary. League membership—the key to regular political activity and to membership on political self-government bodies—is not distributed equally across social groups in Yugoslavia. Rather, it tends to be concentrated fairly heavily in the hands of those from the upper reaches of the socioeconomic scale. In sum, affiliation with the League eliminates differences in activity across social levels *among those so affiliated.* But there are sharp social differences between those who become members of the League and those who do not. And because of this, in the population as a whole there is considerable disparity among social strata in the extent of their participation in regular political activity and in political self-government.

In contrast, the data suggest that there are two paths to participation in workers' councils: political access through the League or access through socioeconomic resources (the paths overlap, of course, in many cases). Of the two, however, the latter plays a more central role. Because of the importance of socioeconomic level in determining who becomes a member of a workers' council, the role of the League is somewhat diminished. It controls access to workers' council membership to some extent—League members are disproportionately likely to be workers' council members—but it does not dominate access to such activity as much as it dominates access to other forms of political activity. The data are consistent with the interpretation that workers' councils open channels for a more technocratically oriented participation, one based on the skills and expertise believed necessary for such activity and less on the political commitments implied in affiliation with the League.

The data in Table 5 bear out this idea. There we compare the proportion of workers' council members among League members and nonmembers in different occupational categories. The last column shows the ratio of the percentage of workers' council members among nonmembers in the League to the percentage among League members. The higher the ratio, the less necessary is League membership for participation in workers' councils. As one can see, League membership plays the biggest role among the unskilled blue collar workers. It plays a smaller role for skilled blue-collar workers and a still smaller role for white-collar workers, especially those in the highest education level. In other words, if one has the technical skills of the skilled worker or the expertise of the higher-educated white-collar worker, one has less need for political affiliation with the League to participate in a workers' council. Unskilled workers depend most on the League.

Our contention that involvement in workers' councils represents a more technocratic kind of participation than does participation in other aspects of political life has been supported largely on the basis of the social characteristics of the various activist groups and their affiliation with the League. We have no direct

Table 5. Percent Who Take Part in Workers' Councils by Occupation and League Membership (Eligible Population as Base)

Occupation	Non-League	League	Ratio Non-League/ League
Blue-collar			
Unskilled, semi-skilled	20%	45%	.44%
Skilled, highly skilled	39	63	.62
White-collar			
Low education	31	46	.70
Medium education	37	58	.64
High education	57	67	.85

evidence that different participants think differently—i.e., that some are more technically oriented and others more politically oriented. Unfortunately our study contains no data on this. But we can present some indirect evidence that these groups are different in how they think about political versus technical criteria.[21] We asked a question about the ordinary citizen's obligations to his country, coding answers into numerous categories. In Table 6 we present the proportions of the various activist types who gave one of the following answers as the main duty of the citizen:

a. the duty to obey the law.
b. the duty to take an active part in the political life of the nation.
c. the duty to do one's job well.[22]

If workers' council members are more "technocratic" in orientation while those active in political self-government or in regular political activity are more politically oriented, one should see this reflected in their choice of citizen duty. The data in Table 6 offer some support for this position. Compare the two self-government activist groups. A greater proportion of political self-government members than workers' council activists mention obedience to the law and active participation as important civic obligations. A greater proportion of workers' council members than political self-government participants cite conscientious work as an important obligation. Indeed, a greater proportion of workers' council members than either regular political activists or political self-government activists choose conscientious work as an obligation. Moreover, workers' council members more frequently choose doing one's job well than other obligation as a primary citizen duty.

Conclusion

We have considered membership in workers' councils from two perspectives: the socioeconomic representativeness of that membership and the process of mobilization to workers' council activity. We do not claim that these exhaust the important questions that can be asked about such participation, but they do focus on an important aspect of this participatory institution. One of the major arguments for industrial democracy is that it opens up opportunities for participation for those who would not otherwise be active, and since that participation is within the workplace, it greatly increases the average person's control over his or her life.

Our findings indicate that workers' councils provide participatory opportunities for a large proportion of the work force; about two out of five workers in the socialist sector report having been members of such a council at one time or another and indeed, one out of five of the entire sample reports such a membership. In this sense, workers' councils provide participatory opportunities on continuing bodies for a larger proportion of the population than would be possible if self-governing bodies were limited exclusively to the political arena. The figures are substantially higher than the nine percent who report membership in political self-government councils.

Access to membership in workers' councils, however, is not equally available to all Yugoslavs. Only part of the citizenry is in a position to take advantage of this participatory channel. Those who are not employed in the socialist sector have no such opportunity (though some have opportunities to participate in other functional self-management bodies such as apartment house committees). Furthermore, those who are selected out of the eligible pool are not representative of that pool; the more skilled and better-educated workers have a greater chance of becoming council members.[23]

Table 6. Citizen Duty by Participant Type

Participant Type	Percent Saying Main Citizen Duty Is: Obedience to Law	Active Political Obligation	Conscientious Work
Regular political activists	18%	17%	21%
Workers' council members	22	21	35
Political self-government activists	24	27	31

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[21] For further analysis of differences in political attitudes among the various participant groups, see Shabad, Ch. 9.

[22] Other possible responses were to serve in the armed forces and to act in a moral way.

[23] See footnote 14.

The recruitment process to workers' councils is best seen in comparison with that to other forms of political activity in Yugoslavia. For all kinds of activity–workers' council activity as well as other kinds–socioeconomic resources play an important role, as does affiliation with the League of Communists. But the relative importance of these criteria differs between participation in workers' councils and other activities. Workers' council activity appears to open channels to those who have status within the context of the workplace. As studies of the occupational composition of workers' councils have repeatedly shown, unskilled workers are greatly underrepresented; skilled workers are overrepresented; and highly educated white-collar employees are the most overrepresented of all occupational categories.[24] League members are more likely to be elected to workers' councils, but this in part reflects the fact that League members come disproportionately from the more skilled and better-educated workers. The important point is that highly skilled and educated nonmembers of the League are readily granted access to workers' councils. In this sense, workers' councils offer an alternative channel to activity that bypasses the League.

For other kinds of activity, League membership appears to be more important than level of skill or education. Again this does not mean that socioeconomic level plays no role, but its impact is channelled through the League. League members are likely to come from the more advantaged groups in Yugoslav society. But those with higher skill and education levels need League membership as a step to other forms of political participation. If they have the former and not the latter, they are low in activity–a situation contrasting with that for membership in workers' councils.

In sum, mobilization to workers' councils differs from other forms of political activity. These differences, however, do not result ultimately in a marked difference in the kinds of people who participate in workers' councils as opposed to other kinds of activity. Neither workers' council membership nor other political activities are equally distributed across social groups in Yugoslavia. In each case, but for different reasons, the advantaged citizen is likely to be active. Considerable inequalities among social and economic groups in their level of participation, regardless of the kind of participation, result from the absence of organizations in the workplace and in the local community which specifically attempt to recruit disadvantaged groups into the political process. The League, because it plays such a key role in mobilization to regular political activity and to participation in political self-government, has the potential for reducing political inequality across socioeconomic groups. It does not do so because of its strong compositional bias toward upper-status citizens.[25] Similarly, were there autonomous trade unions which actively sought to mobilize unskilled and poorly educated workers into the decision-making process within the workplace, membership in workers' councils might be more representative of the labor force as a whole.[26] We can only raise the issue here whether such organizations do not exist in Yugoslavia because, in turn, status groups and conflicts among status groups are given very little official or explicit recognition either in Yugoslavia's socialist democratic ideology or in decision-making processes. To recognize that such conflicting groups exist would be to undermine the image of Yugoslavia as a classless and harmonious society (harmonious in terms of class).[27]

Thus while participatory mechanisms in the workplace do give citizens the opportunity more effectively to influence decisions that affect everyday life in a sphere–the workplace–traditionally closed to citizen participation and control, it does not necessarily ensure a more equitable dispersion of decision making power among various socioeconomic groups. Whether or not such an equitable dispersion is achieved within the workplace depends on who takes part in the new decision-making body.

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[24] See Zivan Tanic, "Social Composition of Workers' Councils in Yugoslavia," *Indian Journal of Industrial Relations*, 3 (July 1967), 27; "The Composition and Some Aspects of the Activity of Self-Management Bodies in Enterprises," *Yugoslav Survey*, 14 (November 1973), 22.

[25] For a discussion of the impact of class-based organization on political inequality in other countries, see Verba, Nie and Kim, *Participation and Political Equality*. See also, Shabad, Ch. 8.

[26] Many Yugoslav social scientists have argued that the absence of autonomous trade unions serves to reinforce the bias of workers' councils toward highly skilled and professional workers. See Vladimir Arzensek, "A 'Conflict Model' and the Structure of Yugoslav Society," *International Journal of Sociology*, 2 (Winter 1972–73), 364–83; Josip Zupanov, "Employees' Participation and Social Power in Industry," *Participation and Self-Management*, Vol. I (First International Sociological Conference on Participation and Self-Management: Dubrovnik, Yugoslavia, December, 1972), 33–40. For data concerning the relationship among union membership, socioeconomic status and membership in workers' councils, see Shabad, Ch. 8.

[27] For a discussion of the impact of class-based ideologies on political inequality in other countries, see Verba, Nie and Kim, *Participation and Political Equality Stratification.* For a discussion of this issue in relation to Yugoslavia, see Adizes, Woodward, and Shabad.

Social Cleavages and Party Choice in Israel: A Log-Linear Analysis*

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This article attempts to fill two gaps in the literature on individual party choice. First, it deals with hitherto unanswered questions about relationships between social cleavages and party choice in Israel. Second, the article attempts to overcome methodological problems arising in the multivariate analysis of multiparty systems by utilizing Goodman's method of log-linear contingency table analysis. In the sample, occupation is not as strongly related to party choice as is a nonhierarchical dimension of economic position, sector of the economy. Ethnicity is modestly related to party choice, but hypotheses that the relationship is affected by place of birth, age, or other variables are disconfirmed. Hypotheses that the relationship between religiosity and party choice is affected by economic position are also disconfirmed. The advantages of using log-linear contingency table analysis are demonstrated.

This article attempts to fill two gaps in the literature on individual voting. The first gap is substantive; although eight national elections have been held since Israel gained independence, little is known about individual party choice there.[1] The focus will be on how party choice is determined by economic position, ethnicity, and religiosity of citizens.[2]

The second gap to be filled is methodological. One weakness of previous work on Israeli voting is shared by most analyses of party choice in multiparty systems, and even by some analyses of two-party systems. The dependent variable, party choice, consists of discrete categories which are difficult to deal with using conventional multivariate data analysis techniques. The attempt here will be to advance the multivariate analysis of multiparty systems by using Goodman's log-linear method of contingency table analysis.[3] As far as the author knows, this is the first use of Goodman's methods to analyze party choice in a multiparty system.[4]

The article will have six parts. The first, the first substantive problem, will outline questions about the relationship of economic position and party choice. The second part will describe the data. The third part will describe weaknesses in traditional data analysis methods, will

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[1] Only one scientific survey of voters has been reported on previously; see Alan Arian, "Electoral Choice in a Dominant Party System," in *The Elections in Israel–1969*, ed. Alan Arian (Jerusalem: Jerusalem Academic Press, 1973), pp. 187–201; Alan Arian, *The Choosing People* (Cleveland: Case Western Reserve University Press, 1973). The published analyses suffer from substantive and methodological flaws; for example, important variables, including sector of the economy, are ignored, analyses are based for the most part on bivariate tables with no calculation of measures of association, and the refusal rate concerning party choice was 30 percent.

[2] The selection of independent variables was influenced by the work of Lipset, Rokkan, and others, who show the widespread importance of such variables and provide a comparative framework into which this work may fit; see S. M. Lipset and Stein Rokkan, "Cleavage Structures, Party Systems, and Voter Alignments: An Introduction," in *Party Systems and Voter Alignments*, ed. Seymour M. Lipset and Stein Rokkan (New York: Free Press, 1967), pp. 1–64; Richard Rose and Derek W. Urwin, "Persistence and Change in Western Party Systems since 1945," *Political Studies*, 18 (September 1970), 287–319.

[3] See, e.g., Leo A. Goodman, "A General Model for the Analysis of Surveys," *American Journal of Sociology*, 77 (May 1972), 1035–86; "Causal Analysis of Data from Panel Studies and Other Kinds of Surveys," *American Journal of Sociology*, 78 (March 1973), 1135–91; and "The Multivariate Analysis of Qualitative Data: Interactions Among Multiple Classifications," *Journal of the American Statistical Association*, 65 (March 1970), 226–56; James A. Davis, "Hierarchical Models for Significance Tests in Multivariate Contingency Tables: An Exegesis of Goodman's Recent Papers," in *Sociological Methodology 1973–1974*, ed. Herbert L. Costner (San Francisco: Jossey-Bass, 1974), pp. 189–231.

[4] Knoke has used the methods to analyze party choice in the United States, but uses only dichotomous variables; see David Knoke, "Religious Involvement and Political Behavior: A Log-Linear Analysis of White Americans, 1952–1968," *Sociological Quarterly*, 15 (Winter 1974), 51–65.

present the advantages of log-linear contingency table analysis, and will explain briefly the method itself. Results of the data analysis will be presented in the fourth part. The last two parts will consist of analyses of the relationships between party choice and ethnicity and religiosity, respectively.

Economic Position and Party Choice

The Problem. Most studies dealing with the relationship of economic position to party choice consider only one economic dimension: class, most often measured in terms of occupation or income.[5] A small body of theoretical and empirical work, however, proposes that other, nonhierarchical dimensions of economic life might become politically important.[6] Lichtheim and Ossowski point out that Marx considered classes in the context of relatively free and competitive capitalist societies. As laissez-faire capitalism declines and changes in social structure can be influenced by decisions of political authorities, however, the situation departs from that described by Marx. Class antagonism may give way before other forms of social cleavage, including economic cleavage organized by the political authorities or powerful groups.

In modernizing nations, in particular, the role of government in allocating resources is often very great, and traditions of class conflict may be weak; in such nations, one might expect economic conflict to take a form different from that common in European and Anglo-American nations.[7] One would expect economic dimensions other than class to be politically important where the government or comparable body controls a great deal of economic resources, where there is political freedom of organized corporate groups trying to affect the allocation of resources, and where the system is flexible enough to permit the rise of new political groups.

One would expect a nonhierarchical dimension of economic life to be politically important in Israel, if anywhere. First, the preconditions for the importance of such a dimension are met. For a long time, quasi-governmental institutions of the Jewish settlement and later Israeli state have played a major role in the allocation of economic resources; the government role in the economy is now very great.[8] In addition, traditions of class conflict within the Jewish community have been weak, and there has been sufficient political freedom for the creation of political and economic organizations.

The obvious organizational focus of economic conflict in Israel is the Histadrut labor federation, which has been a politicized, predominantly leftist nation-building organization performing a wide range of economic and other roles, including allocating capital and employing a significant fraction of the labor force.[9]

The development of the Histadrut apparently led to the existence of two lines of economic cleavage. First, there have been continuing battles over resources and power between the Histadrut and the private sector, with the socialist parties and the Histadrut allied against private enterprise and the nonsocialist, nonreligious parties (since 1948, the public sector has become important). To the extent that income depends upon the allocation of resources between rather than within sectors of the economy, all employees in a given sector would have the same interests—increasing their sector's share of the total resources—regardless of their hierarchical position within the sector. The first hypothesis is thus: *party choice will vary by sector, even when occupation within sectors is controlled.*

The second economic cleavage is the class cleavage; within the private sector, its existence is often taken for granted, with unions representing workers against employers. It is not clear, however, if one should expect class conflict within the public and Histadrut sectors. Thus, there is a question to be answered: *Is party choice correlated with occupation in the private sector but not in the Histadrut or public sectors?*

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[5] E.g., Robert R. Alford, *Party and Society* (Chicago: Rand McNally, 1963); Richard F. Hamilton, *Class and Politics in the United States* (New York: Wiley, 1972).

[6] This argument draws on: Lipset and Rokkan, "Cleavage Structures"; Raymond J. Murphy and Richard T. Morris, "Occupational Situs, Subjective Class Identification, and Political Affiliation," *American Sociological Review*, 26 (June 1961), 383–92; Morris Janowitz and David R. Segal, "Social Cleavage and Party Affiliation: Germany, Great Britain, and the United States," *American Journal of Sociology*, 72 (May 1967), 601–18; and especially George Lichtheim, "Class and Hierarchy: A Critique of Marx?" *European Journal of Sociology*, 5 (1964), 101–11. Relationships between economic groups are non-hierarchical when there is no obvious way to order them relative to each other in terms of "less" and "more" or "higher" and "lower"; economic sectors as used in the census fit the definition.

[7] Lichtheim; the possibility of economic conflict involving nonhierarchical dimensions is an implied criticism of Dahrendorf's model of conflict; see Ralf Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford: Stanford University Press, 1959).

[8] Moshe Burstein, *Self-Government of the Jews in Palestine since 1900* (Tel Aviv, 1934); S. N. Eisenstadt, *Israeli Society* (New York: Basic Books, 1967).

[9] Although the impact of sector on political behavior has been discussed informally in work on Israel, neither Arian's work nor any other actually measures its impact.

The focus will be on the three largest parties—the Alignment, Gahal, and the National Religious Party INRP), which together received over three-fourths of the vote cast in the election. The Alignment is the social democratic party which (counting its predecessors) has led every government in Israeli history, and is by far the largest party, receiving 46.2 percent of the votes cast in the 1969 election. Gahal is the center-right party which has consistently led the opposition to the Alignment (when predecessor parties are included).[10] In 1969, it received 21.7 percent of the vote in the population. The NRP is a moderate religious party which has consistently received about ten percent of the votes cast, and has been a consistent coalition partner of the Alignment (which has never gotten a majority of the votes cast). Nonvoters are included as well. Smaller parties are not examined because the numbers included in the sample were very small.

Previous Work. Most previous work on the relationship between economic position and party choice consists of informed speculation. It has been assumed that with regard to the relationship between occupation and party choice, the Alignment should receive especially strong support from manual laborers, since it is a labor party. Professionals in Israel tend to feel that Alignment policies cause them to have incomes lower than they deserve relative to other groups;[11] they seem least likely to vote for the Alignment. In addition, small businessmen are seen as likely to oppose the Alignment.

With regard to sector, previous work suggests that Gahal is the representative of the private sector, with the Alignment being strongly supported by workers in the Histadrut sector. But real answers to the specific questions posed are not found in previous work.

Data

The data analyzed refer to the 1969 Israeli national elections. The sample consisted of 732 Jewish urban men who were eligible to vote in the 1969 election, 633 in Haifa and 99 in Jerusalem. Quota sampling was used; the demographic characteristics of the sample are approximately those of the Israeli adult male Jewish population, except for some overrepresentation in the sample of Jews from Eastern European countries (and, of course, location of residence—for more details, see appendix on the sample).

Methods

The hypothesis and question just formulated present immediate problems for the data analyst using conventional multivariate techniques. First, the dependent variable—party choice—has four unordered categories (those voting for Alignment, Gahal, NRP, and those nonvoting); thus, regression analysis cannot be applied directly. Second, the question about the relationship between occupation and party choice suggests not only that there may be an interaction between sector and occupation as determinants of party choice, but also that the interaction takes a very specific form: occupation may be related to party choice within the private sector, but not in other sectors. Specific hypotheses about the forms of interaction are also difficult to test using conventional techniques.

These two problems—unordered dependent variables and interactions between independent variables, particularly the former—have been major stumbling blocks in the multivariate analysis of multiparty systems. The problems have had such widespread impact that a fairly long exposition on the methodological problems and a proposed solution is in order here.

Although a number of good multivariate analyses of party choice in the United States have been published, relatively few similar studies on multi-party systems are available. One important reason is methodological. The most common statistical technique used in multivariate analyses of voting is dummy variable regression, where the dependent variable, party choice, has two categories. In the United States, such a procedure is reasonable, since in most elections there are only two important parties; even so, nonvoting and voting for third parties are often left out of analyses. Attempts have been made to analyze multiparty systems using regression analysis or other variants of the general linear model, but either the dependent variable is dichotomized on the basis of a priori assumptions or a series of regressions is run, each equation dealing with one pair of parties.[12] Both procedures are unsatisfactory because they make it impossible to consider the impact of independent variables on party choice as it really exists—as the choice among a number of discrete alternatives considered simultaneously.[13]

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[10] See, e.g., Peter Y. Medding, *Mapai in Israel* (London: Cambridge University Press, 1972), Ch. 3.

[11] In the 1973 election, Gahal joined forces with two other right-of-center parties to form Likud.

Log-linear contingency table analysis can be a much more satisfactory method than those used previously for the analysis of multiparty elections (which, in 1948 and 1972 included the American presidential election, considered realistically). Essentially, it is a method whereby approaches and procedures analogous to those developed to deal with continuous variables in the general linear model are employed to estimate cell counts in contingency tables. Using the methods, it is possible to measure the impact on cell frequencies in *n*-way contingency tables of marginal distributions, zero-order relationships, and all possible interactions among the variables.

The approach to be used is Goodman's.[14] Obviously an extended exposition of the approach would be impossible here; Davis devotes hundreds of pages to his useful attempts to explain Goodman's system in language less technical than Goodman's.[15] Goodman (and others) have developed two related systems of analysis for dealing with *n*-way contingency tables—the first system is for testing the statistical significance of relationships, and the second is for measuring their magnitudes.

To test the statistical significance of relationships, the frequencies expected in cells of the contingency tables are predicted in an iterative procedure using a series of parameters pertaining to the marginal distributions of the variables and their interactions.[16] In a three-way contingency table, for example, call F_{ijk} the expected frequency in cell (i,j,k). Calling the three variables A, B, and C, the F_{ijk} can be calculated in terms of the following parameters: τ^A, τ^B, τ^C, which represents the "main effects" of each variable, or their marginal distributions; τ^{AB}, τ^{AC}, τ^{BC}, which represent the two-way interactions between pairs of variables; and τ^{ABC}, which represents the three-way interaction. The basic inputs into the system are odds ratios and conditional odds ratios, the ratios between cells-counts under varying conditions. When all the possible parameters are used to calculate F_{ijk}, expected and actual cell frequencies will be identical, and chi-square for the table will equal zero; this is the saturated model, expressed as

$$F_{ijk} = \tau^A \tau^B \tau^C \tau^{AB} \tau^{AC} \tau^{BC} \tau^{ABC} \qquad (1)$$

The multiplicative model is transformed into an additive one by taking the natural logarithm of each element.

The question then is whether the predicted and actual cell counts would still be similar (as measured by likelihood-ratio chi-square) after one or more of the parameters are removed from the equation; the aim is to find the most parsimonious model which still fits the data well.[17] Note that since the aim is to fit the actual data well, the aim is to find models with *low* chi-square values, and hence high *p*-values. In the usual significance testing, low *p*-values are usually "desired," so the treatment of chi-square values in the Goodman procedure is somewhat counter-intutive.

Each model to be tested can be written like equation (1) or its log-linear equivalent, including or leaving out various parameters, each representing an effect of a marginal or interaction. The models are "hierarchical" in the following sense: whenever a more complex effect is included in the equation (for example, a two-way interaction), its simpler components

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[12] E.g., Janowitz and Segal, "Social Cleavage and Party Affiliation"; Arian, *Choosing People;* Naomi Kies, "Constituency Support and the Israeli Party System" (Ph.D. dissertation, M.I.T., 1969).

[13] Other methods, such as multiple discriminant function analysis, could be employed, but they too present problems. In multiple discriminant function analysis, for example, prediction of party choice is possible, but substantive interpretation of the coefficients is difficult because the predictor variables are not the original variables but rather linear combinations of them. See, e.g., Paul Burstein, "Social Structure and Politics in Israel" (Ph.D. dissertation, Harvard University, 1973).

[14] See Goodman cited in footnote 3. Log-linear contingency table analysis is relatively new, and competing but very similar systems also exist; see Marc Nerlove and James Press, *Univariate and Multivariate Log-linear and Logistic Models* (Santa Monica: RAND, 1973).

[15] E.g., James A. Davis, "Hierarchical Models"; James A. Davis, "The Log Linear Analysis of Survey Replications," in *Social Indicator Models,* ed. Kenneth C. Land and Seymour Spilerman (New York: Russell Sage Foundation, 1975), pp. 75–104.

[16] Contrary to usage in some areas, variables are said to "interact" whenever they are related; thus, variables can interact in a two-way table.

[17] Conceptually, the procedure is somewhat analogous to the elimination of possible causal paths in path analysis, where one aim is to determine which paths can be set to zero while still permitting reproduction of the original coefficients; see Leo A. Goodman, "Causal Analysis of Data"; Leo A. Goodman, "The Analysis of Multidimensional Contingency Tables When Some Variables are Posterior to Others: A Modified Path Analysis Approach," *Biometrika,* 60 (1973), 179–92.

(in this case, the marginal distributions of the two variables) are necessarily included as well. Thus, in a model involving four variables, *A, B, C, D,* a model involving the three-way interaction *(ABC)* necessarily includes the component two way interactions *(AB), (AC), (BC),* and the marginals *(A), (B), (C).* Thus, it is possible to test a series of models to determine which most parsimoniously provides a good fit to the data; models can be compared to each other to see whether the addition of specific parameters improves the fit significantly.

Once it has been determined which model best fits a given set of data, it is possible to use the effect parameters (usually the lambdas of the log-linear form) from that model to gauge the strengths of relationships. There are problems in doing so, as Davis points out: the effect measures lack intuitive meaning, especially when the variables have more than two categories. But there are advantages to examining the parameters, even if only to determine the relative magnitudes of effects. Using the parameters, it is possible to condense the multiple subtables of an *n*-way table into a relatively small number of coefficients, so the relationships between categories of variables can be gauged controlling for the effects of whatever other variables are in the model.

Log-linear contingency table analysis thus should have several advantages over other methods in studying multiparty systems. It is possible to deal with more than two parties simultaneously (and with categorical independent variables as well, of course, though they could also be treated as dummy variables in regression equations). Hypotheses involving three-way interactions can easily be tested. It will be possible to examine relationships among categories of variables in much more detail and much more easily than previously. The technique does have disadvantages beyond the problems of interpreting effect parameters. Davis mentions some.[18] In addition, this technique, like all others using contingency tables, runs quickly into the problem of empty cells. Consequently, the models which follow involve no more than three variables simultaneously, due to the modest sample size. Note also that though intuitive causal interpretations will be obvious, the statistics themselves describe merely interdependencies.[19]

Economic Position and Party Choice: Findings

Table 1 presents the findings relating sector and occupation to party choice. Expectations based upon previous work are generally confirmed; workers employed in the Histadrut sector and manual workers are the strongest supporters of the Alignment, while private sector workers are the ones most likely to support Gahal. Professional and managerial workers are less likely than others to support the Alignment.[20]

In the Goodman system of hierarchical significance testing, a three-variable system (such as party choice-occupation-sector) produces 18 possible models to be tested. Some of these models, however, are implausible or uninteresting in the present context; only eight models are potentially interesting.[21] Table 2 presents the results of testing the eight models. In the notation used, *(P)* means the effect of the marginal distribution of party choice, *(PO)* means the association of party and occupation, and *(POS)* means the interaction of all three variables. As stated above, *(PO)* subsumes *(P)* and *(O)*.

The first conclusion is that occupation is related to party choice in the private sector, and in the public and Histadrut sectors as well. The hypothesis that occupation is related to party choice only in the private sector implies a statistically significant three-way interaction among the variables; in the Goodman parlance, the saturated model *(POS)* (which subsumes simpler models) would fit the data significantly better than the model involving only the two-way interactions, *(PS) (PO) (OS)*.

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[18] Davis, "Hierarchical Models," "Log Linear Analysis."

[19] It is theoretically possible to perform analyses comparable to path analyses, but doing so with polytomous variables is extremely laborious and complicated, and probably not worth the effort in the data set used below; see Goodman, "Causal Analysis"; "Analysis of Multidimensional Contingency Tables."

[20] The Alignment is the modal choice of every group; it is merely *relatively* strong or weak in different groups in the sample. More detailed data including a finer breakdown of occupations and data for all parties do not contradict Table 1; both occupation and sector are related individually to party choice at better than the .001 level.

[21] The 18 models consist of every logically possible combination of effects which would cause the cell frequencies to be unequal to each other, including the effects of the marginal distributions, of two-way relationships, and of higher order relationships, and all possible combinations thereof, plus the null model in which all cell counts are equal. (The number of models thus increases very rapidly with the number of variables; a four-variable system includes over a hundred possible models; obviously substantive considerations should be used *a priori* to select among the possibilities.) We eliminate ten possible models on the basis of our *a priori* knowledge that cell counts are not expected to be equal to each other, by our lack of interest in effects which are caused merely by skewed marginals on one or two of the variables, and by our lack of substantive concern with relationships not involving party choice.

To determine which of two models fits the data better, the chi-square values of the two models are compared. The difference between the chi-square values of the two models is then treated itself as a chi-square statistic and tested for statistical significance. If the difference is significant, the model with the better fit to the data (i.e., the one with the lower chi-square value) is considered the better model (though it will be less parsimonious).

In this case, the chi-square value for the model *(PS) (PO) (OS)* is 9.74 with 12 degrees of freedom; of course, chi-square for the saturated model is zero. The difference between the two is 9.74 with 12 degrees of freedom (the difference in degrees of freedom between the two models). This chi-square value is not statistically significant at .05 or better, so the hypothesis that there is a statistically significant three-variable interaction can be rejected.

The second question to be considered is whether sector is related to party choice, controlling for occupation; that is, is the model *(PO) (PS)* a significant improvement over the model *(PO) (S)*? The answer is clearly yes. Taking the difference between the chi-square values for the models, (45.95–21.89) and gauging its significance for 6 degrees of freedom (the difference in degrees of freedom between the two models, 22–16), the model including the interaction between party and sector is an improvement over the simpler model at the .01 level. In this set of data on Israel, at least, a non-hierarchical economic dimension *is* related to party choice even after occupation is taken into account.

If one wants to choose the one "best" model from among the eight, one sees that statistical criteria alone are insufficient. Explanatory power must be balanced against parsimony; one model might provide a slightly better fit to the data, but at the cost of added complexity, and

Table 1. Percent Choosing Party by Occupation and Sector

	Party			
Occupation	Alignment	Gahal	NRP	None
Private Sector				
Manual	60.0 (75)	24.0 (30)	5.6 (7)	10.4 (13)
Lower nonmanual	40.0 (16)	30.0 (12)	12.5 (5)	17.5 (7)
Professional, managerial	48.4 (31)	32.8 (21)	6.3 (4)	12.5 (8)
Histadrut Sector				
Manual	85.2 (46)	7.4 (4)	3.7 (2)	3.7 (2)
Lower nonmanual	100.0 (11)	0 (0)	0 (0)	0 (0)
Professional, managerial	64.3 (9)	21.4 (3)	0 (0)	14.3 (2)
Public Sector				
Manual	67.9 (72)	18.9 (20)	4.7 (5)	8.5 (9)
Lower nonmanual	70.3 (26)	8.1 (3)	13.5 (5)	8.1 (3)
Professional, managerial	60.0 (51)	16.5 (14)	8.2 (7)	15.3 (13)

Table 2. Models for Party Choice (P), Sector (S), and Occupation (O)

Model	Likelihood-Ratio Chi-Square[a]	P-Value	Degrees of Freedom
(P) (O) (S)	57.6	.001	28
(PO) (S)	45.95	.002	22
(PS) (O)	33.57	.054	22
(PO) (PS)	21.89	.15	16
(PO) (OS)	32.96	.02	18
(PS) (OS)	20.58	.30	18
(PS) (PO) (OS)	9.74	over .5	12
(POS)	0	–	0

[a] In calculating significance tests, cell counts were increased by .5 to eliminate empty cells, as Goodman suggests.

the researcher might have reasons to prefer the simpler model. The saturated model is of course best in a trivial sense, but the model *(PS) (PO) (OS)* is probably preferable because it is simpler and the saturated model is not significantly better. Of the models not involving all two-way interactions, *(PS) (OS)* seems to be the best, in the following sense. The chi-square value is slightly lower than in the model *(PS) (PO)* (though not significantly); in addition, the model *(PS) (OS)* fits the data worse than the model *(PS) (OS) (PO)* at only the .10 level, approximately. In other words, leaving occupation out entirely as directly related to party choice does worsen the fit, but not that much. On the whole, then, sector is significant in the three-variable system, and it is even possible that occupation could be left out of the model as directly related to party choice without excessively worsening the fit; of course, one would want to recheck such a conclusion with other data sets.

To gain further insight into the details of the relationships among the variables, it is possible to examine the lambda coefficients within particular cells of the tables. The lambdas of substantive interest are those measuring the relationship between party choice and other variables in the particular model selected for analysis. For the sake of simplicity, the results presented are those for the model *(PS) (OS)*; thus, the relationship between occupation and party choice is not considered.[22] Table 3 presents the lambda and standardized lambda coefficients. As mentioned above, it is difficult to interpret absolute magnitudes substantively. Nevertheless, by examining the relative magnitudes and statistical significance of effects within various cells, it is possible to study the relationships between particular categories. Doing so provides a richer substantive interpretation of the data than is possible with significance testing of models only. Examining the coefficients has advantages over looking at the original three-way table because the original tables are collapsed into a single table and the effects are tested for significance.

Table 3 shows only three effects that could be considered statistically significant.[23] As expected, citizens in the private sector favor Gahal disproportionately and are disinclined to vote for the Alignment. Citizens in the Histadrut sector disproportionately favor the Alignment. Given how little has been known about Israeli voting behavior, the nonsignificant effects are also interesting. Sector primarily distinguishes between Alignment and Gahal voters; sector is only very weakly related to voting for the NRP or nonvoting. In addition, workers in the public sector do not favor any party in particular; this is something of a surprise, given the purported importance of party connections in hiring in some ministries and the long-term Alignment and NRP control of the government. In any case, there is now some evidence not only that sector is related to party choice, but that in particular the difference between private and Histadrut enterprises is related to the choice of the Alignment as opposed to Gahal.[24]

Table 3. Lambda Effects for Sector and Party in Best-Fitting Model Involving Party Choice, Sector, and Occupation-Model (PS) (OS)

	Party			
Sector	Alignment	Gahal	NRP	None
Private				
Lambda	−.389	.323	−.006	.071
Standardized Lambda	−3.174	1.976	−.027	.367
Histadrut				
Lambda	.487	−.223	−.136	−.128
Standardized Lambda	2.696	−.854	−.378	−.424
Public				
Lambda	−.098	−.100	.142	.057
Standardized Lambda	−.807	−.587	.615	.288

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[22] Confirming the impressions derived from the significance testing of various models, no single relationship between party and occupation is strong, and the relationship between sector and party choice is not changed when the model including occupation is examined.

[23] That is, where the coefficient is significantly different from zero at the .05 level or better.

[24] It might be helpful at this point to make comparisons between the analyses presented and results found using more conventional techniques.

Ethnicity and Party Choice

The Problem. Ethnicity is potentially the most important cleavage considered here, because it is the most problematic social cleavage within the Jewish community. Economic and religious issues have been bitterly contested, but their political meaning and form have long been institutionalized.

The recognition of ethnicity as a potentially very divisive factor is relatively recent, however, and its political implications have not been fully worked out. Immigrants from non-western countries typically had less education than western immigrants, seldom had experience in modern industrial or bureaucratic jobs, and knew little of political Zionism or the type of politics practiced in Israel. The Oriental (non-western) Jews tended to be economically disadvantaged and politically powerless; the differences between them and the European Jews tended to continue in the second generation. Although the Orientals have made economic and political progress in recent years, the conjunction of poverty, powerlessness, and ethnicity seems likely to promote political division between ethnic groups.

Though the (uncontrolled) relationship between ethnicity and party choice is important by itself, we must also consider the possibility that the relationship is affected by third variables. There are several potentially important possibilities. First, the relationship between ethnicity and party choice may be changing as generations change; especially important is the possibility that younger or native-born Orientals are particularly opposed to the Alignment. Of course, the data were collected at a single point in time, so true analysis of trends cannot be carried out. Nevertheless, it is possible to determine whether the relationship between ethnicity and party choice is affected by nativity or by the age of the respondent. Second, the relationship between ethnicity and party choice may be affected by the fact that Orientals tend to be in inferior occupations, which, as noted above, show a slight tendency to favor the Alignment. Finally, Orientals tend to be more religiously observant than Europeans, although they are becoming less so with the passage of time; thus, it would be useful to know the relationship of ethnicity and party choice with religiosity taken into account.

Thus, questions about the relationship between ethnicity and party choice in Israel to be considered are: what is the uncontrolled relationship? Is there any evidence that the relationship is different among those born in Israel opposed to those born abroad, or among different age groups? Is the strength of the relationship affected by economic position or religiosity?

Previous Work. Surprisingly little is known about the impact of ethnicity on party choice. Available findings tend to indicate only a slight impact of ethnic differences on national politics. No ethnic lists have won any seats in national elections since 1951. Ecological and survey data apparently indicate that to some extent, Orientals vote for Gahal and Europeans for the Alignment.[25] The previous findings suffer from a number of weaknesses, however. The ecological correlations are often based on quite large units; all the work done is statistically quite simple; and most important, there has been no attempt to consider the relationship between ethnicity and party choice while taking into account the potentially important

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Space limitations preclude detailed presentation of findings, but analyses parallel to all those presented here were carried out using dummy variable regression and multiple discriminant function analysis. Results of the regression analyses were not inconsistent with those described in this article; for example, occupation is not significantly related to party choice once sector is controlled, in four of the six equations (one equation for each combination of categories of the dependent variable).

However, the use of log-linear contingency table analysis gives the researcher three advantages over the user of standard regression techniques, two substantive and one practical. First, the Goodman system enables one to deal with all relationships as a *system;* one does not have a set of independently calculated regression equations where one has to guess about the relationships among them, but rather an interrelated system of equations. Second, it is easy to determine the nature of interactions among variables using the Goodman system, while it is extremely cumbersome to do so using ordinary regression analysis. Finally, from a practical standpoint, the Goodman system is extremely easy to use, once the model-testing features are understood; testing alternative models using regression analysis is extremely cumbersome by comparison.

Thus, it would have been difficult, though not impossible, to test for the specific interactions hypothesized above among sector, occupation, and party choice. Where many hypotheses involve interactions, as is the case throughout this paper, the Goodman system easily enables one to test hypotheses that would be extremely difficult to test using ordinary regression techniques.

[25] Judah Matras, *Social Change in Israel* (Chicago: Aldine, 1965), Ch. 3; Herbert Smith, "Analysis of Voting," in Arian, *Elections in Israel,* pp. 63–80; Moshe Lissak, "Continuity and Change in the Voting Patterns of Oriental Jews," in Arian, *Elections in Israel,* pp. 264–77; Moshe Lissak, *Social Mobility in Israeli Society* (Jerusalem: Israel Universities Press, 1969); Arian, *Choosing People,* Ch. 3.

variables mentioned above: age, nativity, occupation, and religiosity.

Findings. Table 4 relates ethnicity, place of birth, and party choice. Respondents were considered European or Oriental if either they or their fathers were born in the relevant part of the world; hence an "Israeli" is a person who is native-born of a native-born father. The relationship between ethnicity and party choice is statistically significant, but not strong, and the table confirms the findings of others that Europeans support the Alignment disproportionately; the Oriental vote is divided, and third-generation Israelis are least likely to favor the Alignment.

Table 5 presents the tests of the eight relevant models for each of four three-variable systems; each system includes ethnicity and party choice, which are considered in conjunction with nativity, year of arrival at voting age, occupation, and religiosity.[26] To save space we do not present the original tables. Place of nativity is categorized as Israel or abroad. The variable "period of arrival at voting age" is an attempt to measure the effective period of adult socialization in Israel. The aim was to compare people who have been Israeli voters for the same period of time, rather than people of the same age. It did not seem useful to treat as the same a 40-year-old man who grew up in Israel and one who just arrived. The three categories were as follows: those who first became eligible to vote before 1948, those who became eligible between 1948 and 1958, and those who became eligible more recently, either by reaching age 18 during the time period or by immigrating as adults. Religiosity has four categories, ranging from those who never attend religious services to those who attend once a week or more.

Because the three-variable systems involving period of arrival and nativity speak to the same issue—the possibility of change in the relationship between ethnicity and party choice over generations—they will be considered together. In both cases, the results are comparable (Table 5a and 5b). The models involving all three two-way interactions provide the most parsimonious good fit to the data—that is, the models *(PE) (PA) (EA)* and *(PE) (PN) (EN)*. In both cases, this model fits the data better than simpler models at the .005 level.[27] At the same time, the models involving the three-way interaction—*(PEA)* and *(PEN)*—do *not* significantly improve upon the simpler model in either case. Substantively, this means that the pattern of relationship between ethnicity and party choice is the same for both those born in Israel and those born abroad, and also for those first eligible to vote at different times. These findings provide limited evidence that polarization between ethnic groups is not increasing intergenerationally or among younger voters.[28]

Tables 6a and 6b present the coefficients measuring the strength of relationship between particular ethnic groups and particular parties, controlling for period of coming of age and nativity. The general form of the results is the same as in Table 4. The strongest relationships involve European voting for the Alignment, and the antipathy of third-generation Israelis for the

Table 4. Percent Choosing Party by Ethnicity and Place of Birth

	Party			
Ethnicity	**Alignment**	**Gahal**	**NRP**	**None**
Native Born				
European	57.1 (40)	17.1 (12)	8.6 (6)	17.1 (12)
Israel	33.3 (12)	33.3 (12)	8.3 (3)	25.0 (9)
Oriental	38.9 (14)	27.8 (10)	5.6 (2)	27.8 (10)
Born Abroad				
European	72.7 (210)	13.8 (40)	5.9 (17)	7.6 (22)
Oriental	57.5 (84)	25.3 (37)	6.8 (10)	10.3 (15)

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[26] It would be preferable to consider all the variables simultaneously, rather than in a series of three-variable systems, but the small number of cases precludes doing so.

[27] That is, chi-square for the model *(PE)(PA)(EA)* is 11.20 and that for the best-fitting simpler model, *(PE)(EA)*, is 29.50. The difference is 18.30, which is significant at the .005 level with six degrees of freedom. The procedure for the other three-variable system is analogous.

[28] The data provide no evidence as to the possibility that differences between entire ethnic groups have been changing between elections, of course.

Alignment and their inclination not to vote. As above, the Oriental vote is divided.

Tables 5c–d and 6c–d present the relationships in the models involving ethnicity and party choice along with occupation and religiosity.[29] Results were analogous to those already presented. In each case, the model involving all three two-way interactions was a significant improvement over simpler models, while the model involving the three-way interaction did not fit the data significantly better. In examining the tables of coefficients, two differences between those found in tables 6c–d and tables 6a–b can be noted. First, Europeans not only favor the Alignment, but they also oppose Gahal. Second, the strongest relationship between the Oriental group and any party is found in Table 6d relating ethnicity and party choice while controlling for religiosity, though the coefficient does not reach statistical significance. The coefficient indicates a negative relationship between being Oriental and voting for the NRP (controlling for religiosity). This could indicate that as the secularization of the Oriental community proceeds, the Orientals will move away from the NRP.

Table 5. Models for Party Choice, Ethnicity, and Other Variables

Model	Likelihood-Ratio Chi-Square	P-Value	Degrees of Freedom
a. Party Choice (P), Ethnicity (E), and Nativity (N)			
(P) (E) (N)	145.01	0	17
(PN) (E)	119.39	0	14
(PE) (N)	114.50	0	11
(PE) (PN)	88.88	0	8
(PE) (EN)	16.28	.061	9
(PN) (EN)	21.88	.048	12
(PE) (PN) (EN)	1.41	over .5	6
(PEN)	0	–	0
b. Party Choice (P), Ethnicity (E), and Arrival (A)			
(P) (E) (A)	97.22	0	28
(PE) (A)	68.70	0	22
(PA) (E)	76.90	0	22
(PE) (PA)	48.36	0	16
(PE) (EA)	29.50	.042	18
(PA) (EA)	37.70	.004	18
(PE) (PA) (EA)	11.20	over .5	12
(PEA)	0	–	0
c. Party Choice (P), Ethnicity (E), and Occupation (O)			
(P) (E) (O)	87.41	0	28
(PE) (O)	58.87	0	22
(PO) (E)	75.32	0	22
(PE) (PO)	46.78	0	16
(PE) (EO)	30.80	.030	18
(PO) (EO)	47.26	.000	18
(PE) (PO) (EO)	14.55	.267	12
(PEO)	0	–	0
d. Party Choice (P), Ethnicity (E), and Religiosity (R)			
(P) (E) (R)	183.45	0	39
(PE) (R)	151.55	0	33
(PR) (E)	83.45	0	30
(PE) (PR)	51.55	.001	24
(PE) (ER)	120.52	0	27
(PR) (ER)	52.45	.001	24
(PE) (PR) (ER)	18.01	.455	18
(PER)	0	–	0

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[29] The three-variable system ethnicity-party choice-sector was also analyzed. Although ethnicity is related to nativity, period of coming of age, occupation, and religiosity, it is not related to sector; hence, nothing of use could be learned from the system involving sector.

On balance, the relationship between ethnicity and party choice is statistically significant, though it is not very strong. The relationship does not seem to be affected by other variables, and, in particular, does not seem to be changing by generation or age.

Religiosity and Party Choice

The Problem. The religious issue in Israel is primarily one of religiosity rather than religion. Religious issues have been very important ever since the beginning of political Zionism; since 1948, particularly important issue areas have included the role to be played by religion and religious institutions in education and in law, especially personal law.[30] The religious parties have consistently gotten just under 15 percent of the vote in the elections since 1949, with the NRP getting about two-thirds of this. It is fairly widely assumed that most or all of the supporters of the religious parties are religious. It is also widely assumed, however, that not all people who are religious vote for the religious parties. The question is what factors distinguish between religious citizens who vote for the NRP and those who do not. One reasonable hypothesis is that in Israel, as in other countries, "religion" and "labor" are opposed.[31] The Alignment and its affiliated organizations, as well as much of labor organization, try to gain the votes of the observant as well as the non-observant. Thus, one could hypothesize that the observant who are most likely to come

Table 6. Lambda Effects for Party and Ethnicity in Best-Fitting Models

	Party			
Ethnic Group	Alignment	Gahal	NRP	None
a. Party Choice and Ethnicity, Controlling for Nativity-Model (PE) (PN) (EN) Lambda and Standardized Lambda				
Oriental	–.059	.097	–.111	.073
	–.267	.394	.303	.234
Israel	–.334	.191	.093	.050
	–.849	.439	.145	.088
European	.393	–.288	.018	–.124
	1.849	–1.193	.051	–.403
b. Party Choice and Ethnicity, Controlling for Arrival-Model (PE) (PA) (EA)				
Oriental	.193	.089	–.050	–.231
	1.369	.554	–.210	–1.128
Israel	–.675	.157	.065	.453
	–3.586	.822	.228	2.097
European	.482	–.246	–.015	–.222
	4.013	–1.790	–.075	–1.343
c. Party Choice and Ethnicity, Controlling for Occupation-Model (PE) (PO) (EO)				
Oriental	0	.164	.057	–.221
	0	1.056	.260	–1.201
Israel	–.534	.130	–.063	.467
	–2.703	.643	–.210	2.172
European	.534	–.294	.005	–.246
	4.268	–2.042	.027	–1.524
d. Party Choice and Ethnicity, Controlling for Religiosity-Model (PE) (PR) (ER)				
Oriental	.128	.154	–.354	.072
	.652	.751	–.718	.311
Israel	–.561	.243	.045	.274
	–2.084	.900	.071	.868
European	.433	–.396	.309	–.346
	2.595	–2.204	.776	–1.663

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[30] See, e.g., Ervin Birnbaum, *The Politics of Compromise: State and Religion in Israel* (Rutherford, N.J.: Farleigh Dickinson University Press, 1970).

[31] See, e.g., Klaus Liepelt, "The Infra-Structure of Party Support in Germany and Austria," in *European Politics*, ed. Mattei Dogan and Richard Rose (Boston: Little, Brown, 1971), pp. 183--202; Arend Lijphart, *The Politics of Accommodation* (Berkeley: University of California Press, 1968), Ch. 2.

under labor influence, particularly manual workers and those in the Histadrut sector, would be less likely to vote for the NRP than the observant who are in other occupations or sectors. Put another way, this hypothesis is that in the three-variable systems, party choice-religiosity-sector and party choice-religiosity-occupation, the three-way interaction effect will be statistically significant. Thus, the aim in this section is to examine the relationship between religiosity and party choice, and to determine how the relationship is affected by sector and occupation.

Previous Work. Previous work indicates that religious parties do get virtually all their support from the religious, but that, as mentioned, not all who are religious support such parties.[32] In addition, Arian provides some indirect evidence that there is an interaction between religiosity and membership in Histadrut-affiliated trade unions in determining voting for the Alignment, with contact with the labor union working against religiosity.[33] The previous work makes the expectation of interaction effects somewhat more plausible, while not precisely answering the questions posed.

Findings. Table 7 presents the relationship between party choice and religiosity, controlling for sector. Previous findings are confirmed; the NRP gets virtually no votes from any group but the most observant, while the other parties get voters from all levels of religiosity. In addition, the percentages provide some evidence for the hypothesis that there is a division among the observant between those who are in the Histadrut sector, and everyone else. Religious workers in the Histadrut sector are less likely to vote for the NRP than are other religious citizens.

Table 8 shows, however, that while there may be trends in the expected directions, they are not statistically significant in this sample. In the three-variable system involving party choice, religiosity, and sector, the most parsimonious good-fitting model is the model *(PR) (PS);* inclusion of additional interactions, including the three-way interaction is not needed to provide good fit. Thus, the hypothesis is not supported, and we are left not knowing what distinguishes religious citizens from each other with regard to party choice.

Table 9 presents the relationship between religiosity and party choice, controlling for occupation and sector. The findings are very similar in both cases. The least observant group is disinclined to vote. As expected, the most religious group is highly likely to vote for the NRP and disinclined to make other choices. It is also interesting to note that Gahal tends to receive support disproportionately from among those who attend synagogue with some frequency but less than once a week. This relationship would no doubt be interpreted by observers of the Israeli scene as indicating a relationship between being religious and being

Table 7. Percent Choosing Party by Religiosity and Sector

Religiosity–Synagogue Attendance	Party Alignment	 Gahal	 NRP	 None
Private Sector				
Never	51.7 (31)	25.0 (15)	1.7 (1)	21.7 (13)
High Holidays	58.2 (32)	25.5 (14)	0 (0)	16.4 (9)
Sometimes	61.5 (40)	35.4 (23)	0 (0)	3.1 (2)
Once a week or more	39.7 (25)	20.6 (13)	25.4 (16)	14.3 (9)
Histadrut Sector				
Never	80.0 (16)	15.0 (3)	0 (0)	5.0 (1)
High Holidays	80.8 (21)	7.7 (2)	0 (0)	11.5 (3)
Sometimes	93.8 (15)	6.3 (1)	0 (0)	0 (0)
Once a week or more	82.4 (14)	5.9 (1)	11.8 (2)	0 (0)
Public Sector				
Never	68.4 (39)	12.3 (7)	0 (0)	19.3 (11)
High Holidays	72.7 (40)	14.5 (8)	0 (0)	12.7 (7)
Sometimes	67.2 (41)	26.2 (16)	1.6 (1)	4.9 (3)
Once a week or more	54.4 (31)	10.5 (6)	28.1 (16)	7.0 (4)

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[32] Arian, "Electoral Choice."

[33] The statistical technique used by Arian—AID—and his choice of a dependent variable—voting for the Alignment as opposed to all other parties—makes a more precise statement impossible; see Arian, *Choosing People,* Chs. 3–5.

"right wing" in a number of areas (including foreign affairs), but such conclusions are of course speculative.

The analysis buttresses some findings of others, brings evidence to bear on a number of hypotheses not tested previously, and demonstrates the usefulness of log-linear contingency table analysis.

In conformity with previous findings, the data show that manual workers tend to favor the Alignment slightly, that ethnic divisions as expressed in party choice are statistically significant but not great in magnitude, and that the NRP draws its votes almost exclusively from among the religiously observant.

Table 8. Models for Party Choice (P), Religiosity (R), Sector (S), and Occupation (O)

Model	Likelihood-Ratio Chi-Square	P-Value	Degrees of Freedom
a. Party Choice, Religiosity, and Sector			
(P) (R) (S)	127.69	0	39
(PR) (S)	35.15	.237	30
(PS) (R)	103.84	0	33
(PR) (PS)	11.30	over .5	24
(PR) (RS)	31.37	.144	24
(PS) (RS)	100.07	0	27
(PS) (PR) (RS)	7.89	over .5	18
(PRS)	0	–	0
b. Party Choice, Religiosity, and Occupation			
(P) (R) (O)	122.72	0	39
(PR) (O)	32.80	.331	30
(PO) (R)	110.41	0	33
(PR) (PO)	20.43	over .5	24
(PR) (RO)	18.47	over .5	24
(PO) (RO)	96.08	0	27
(PR) (PO) (RO)	5.69	over .5	18
(PRO)	0	–	0

Table 9. Lambda Effect for Party and Religiosity in Best-Fitting Models

Religiosity– Synagogue Attendance	Party			
	Alignment	Gahal	NRP	None
a. Party Choice and Religiosity, Controlling for Sector-Model (PR) (PS) Lambda and Standardized Lambda				
Never	.008	–.046	–.532	.570
	.039	–.181	–1.002	2.123
High Holidays	.267	.097	–.860	.496
	1.113	.342	–1.349	1.639
Sometimes	.328	.614	–.320	–.623
	1.489	2.415	–.596	–1.738
Once a week or more	–.604	–.665	1.712	–.443
	–3.657	–2.886	5.142	–1.687
b. Party Choice and Religiosity, Controlling for Occupations-Model (PR) (RO)				
Never	.007	–.055	–.516	.563
	.039	–.249	–1.106	2.385
High Holidays	.289	.115	–.857	.452
	1.361	.478	–1.547	1.714
Sometimes	.317	.609	–.339	–.587
	1.592	2.773	–.714	–1.873
Once a week or more	–.614	–.669	1.712	–.429
	–4.062	–3.467	5.878	1.882

The analysis goes beyond previous work in the following ways. The finding that sector of employment is related to party choice even when occupation is controlled is significant in the Isareli context. More generally, the finding lends some weight in this specific case to the speculations of Lichtheim and others that under certain increasingly common conditions, class may be superseded as the main dimension of economic conflict by other, organized bases of conflict.

The data show that the relationship between ethnicity and party choice is the same for those born in Israel and those born abroad, and for those who came of voting age in different periods. The relationship between ethnicity and party choice may be changing over time, but the data provide no evidence that such change is a generational phenomenon. In addition, the relationship between ethnicity and party choice remains statistically significant when even a number of variables correlated with ethnicity are controlled—place of birth, period of coming of age, occupation, and religiosity. On balance, of course, most of the relationships between specific parties and ethnic groups are weak; notably, Orientals favor no party in particular. The data provide no explanation for such findings in a situation where there are a number of reasons to expect substantial ethnic conflict; the explanation may lie to some extent in the activities of the parties and unions, which cannot be explored here.

Finally, the data did not support hypotheses that the religiosly observant who vote for the NRP could be distinguished from those who do not by occupation or sector. Other factors must be discovered to distinguish among the observant.

The analysis and hypothesis-testing was greatly facilitated by the use of log-linear contingency table analysis; in many of the cases, in fact, it is not clear what alternative technique would have permitted testing these hypotheses. The technique should be of substantial use in analyzing party choice in multiparty systems.

Appendix: The Sample

Because the sample is obviously not a probability sample of the universe of eligible voters, the results must be interpreted very cautiously. Practical limitations precluded the use of probability sampling in the national population. Sampling was a two-stage process. Because ethnicity and party organization were central foci of the larger study of which this article is a part, neighborhoods were stratified according to ethnicity and party strength to give a full range of combinations of party activity and ethnic group strength. Within neighborhoods, men were sampled on the basis of independent quotas for age and ethnicity, the quotas within each neighborhood reflecting neighborhood composition.

Several factors lend weight to the credibility of the sample and the findings. The demographic characteristics of the sample match fairly closely those of the adult Jewish male population. The patterns of relations among variables within the sample (correlations among sociodemographic variables, friendship and media use patterns, etc.) conform closely to previous Israeli findings. Where it was possible to compare results from the present research with other work in Israeli politics, notably Arian's,[34] the findings are comparable where similar relationships in the data are being considered (Arian does not consider all the variables dealt with here). The use of an entirely urban sample should not affect the results materially, as most Israelis live in the metropolitan areas of the three largest cities, a small proportion of the population is actually rural, and there has been virtually no development of regional cultures in Israel, in politics or other domains.

About 18 percent of eligible people approached refused to be interviewed, and of those interviewed, about 15 percent refused to report their 1969 vote. Both rates of refusal are somewhat higher than in the United States or Great Britaih, but are comparable to those for continental European surveys.[35] Refusals to answer the question about voting were not significantly correlated with income, education, age, occupation, or ethnicity, and refusals at the door did not vary significantly by area. There was some tendency for a higher than expected proportion of voters to report having voted for the Alignment, the dominant party. National results for major groups in the election (and the rough proportions expected on the basis of sampling) were as follows (excluding parties not receiving votes from a significant number of Jews), with the survey results in parenthesis: Alignment, 39.9% (57.8); Gahal, 18.7% (17.8%); religious parties, 12.8% (8.3%); other parties, 8.5% (5.0%); nonvoters, 19.6% (10.9%).

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[34] Arian, *Elections in Israel, Choosing People.*

[35] See Arian, *Elections in Israel,* p. 188; Edward O. Laumann, *Bonds of Pluralism* (New York: Wiley, 1973), p. 18.

Eric Voegelin's Framework for Political Evaluation in His Recently Published Work*

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Eric Voegelin's evaluation of politics is grounded in the symbols and experiences which illumine man's participation in the process of reality. The trail of symbols and equivalent experiences which constitute history provides the fundamental criteria by which the language of politics is to be evaluated. The central philosophical symbol is the Between (metaxy), *coined by Plato. It indicates that human existence is a tension between the poles of the divine height and the cosmic depth. Politics and its language must acknowledge the limitations of this tension: that the human perspective is always from within reality. Existentially, man is a participant, not an observer looking at non-metric reality from the "outside." The existential balance of the open psyche is the product of a perpetual struggle against the tendency to distortions. Such distortions can arise when one or other aspect of reality is allowed to absorb the whole, and individual thinkers attempt to abolish the tension by replacing it with a system which provides the "solution" to the mystery of reality.*

Introduction

If pressed to provide a one-word reply to the question "What serves as Eric Voegelin's framework for political evaluation?", there could be only one valid answer: "Reality." Indeed, what other standard could possibly serve a political science worthy of the name? And yet, here, by raising *the* basic philosophic problem of the nature of reality,[1] we unlock a Pandora's box of difficulties.

True enough, most of us most of the time think we know what "reality" is. For we pride ourselves in being tough-minded and "empirical." Reality in politics, we have heard, is concerned with "who gets what, when, and how," with the struggle to attain prestige, power, and wealth, and with the corruption that so often attends that struggle. But most of us, at least some of the time, are uneasy about accepting this particular "reality" as normative. We think that both individually and as a nation we can do better than that, and part of our objective in describing what is going on is to awaken people to the need to do better than that. We become restless in the Cave, as we experience the pull of a reality beyond its confines.

At this point it would be possible, of course, to call a halt to such questioning by repressing, or ignoring, or denying the pull of a reality beyond that of the immediate political struggle. Such questioning appears unproductive and impossible to "operationalize." And so, too often we put aside the concerns of political philosophy as "irrelevant." Political science is assumed to deal with the "facts"; as for "values," we can derive them from our political culture.

The procedure of "separating" fact and value appears to work reasonably well as long as we can assume that our political culture supplies us with norms whose validity is "self-evident," to quote the immortal words of Thomas Jefferson. For some time now, however, it has become increasingly apparent that the moral consensus which we thought we could assume to pervade our political discourse has been increasingly challenged by the various movements and groups, domestic and international, which, for lack of a better term, we label "extremist." With the shaking of the moral foundation of our surrounding world, we can no longer feel as comfortable about dismissing philosophers and their sustained investigation into the structure and process of reality. At this point, we may even become attentive to what an Eric Voegelin has to say to us.

As I have observed elsewhere,[2] Eric Voegelin did not begin his career as a philosopher; rather,

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[1] Eric Voegelin, *From Enlightenment to Revolution*, ed. John H. Hallowell (Durham, N.C.: Duke University Press, 1975), p. 257.

[2] Dante Germino, "Eric Voegelin: The In-Between of Human Life," in *Contemporary Political Philoso-*

as a teacher of jurisprudence at the University of Vienna, he was led to philosophical speculation because of his experience of the fragility of constitutional democracy in countless European states after World War I. He perceived that institutions rest on ideas, or symbols, of self-interpretation shared by a people, and that if such interpretations go to pieces, the institutions do so as well.

And so Voegelin embarked on his life-long quest for the contours of political reality, a quest which led him from the immediate context of the political struggle in concrete societies to the ideas that animated that struggle in a particular national society to the ideas that bound together a particular civilization and, further, to the comparative structure of political ideas in the various civilizations that have emerged in world history. As his search expanded in scope and time, it also expanded in depth; for the leading or directing ideas of civilizations had to emerge from somewhere, and that "somewhere" turned out to be the depth of the consciousness of concrete, representative human beings. So the domain of "empirical" political reality expanded from constitutions to the ideas that undergird them and, further, from these ideas to the experiences of participation in political and social reality of which the ideas were expressions and finally to the comparative study of experiences of order and disorder in the psyche of representative human types, the philosophers, sages, and prophets who have done the most to illumine the contours of the drama of humanity. Philosophy, then, Voegelin teaches us, is not something optional for a political science worthy of the name: it is the *core* of that science. Political philosophy has to do with the exploration of the contours of political reality: a non-philosophical political science is a contradiction in terms.

In 1974 and 1975 two publications appeared by Voegelin which provide rich documentation for his theory of man, society, and history. One of these works is the long-awaited fourth volume of *Order and History,* which although specifically on developments in the "ecumenic age," from the rise of the Persian to the fall of the Roman Empire, contains many passages, especially in the introduction, refining and developing Voegelin's overall philosophy. Another volume, edited by John H. Hallowell of Duke University, called *From Enlightenment to Revolution,* is a portion of an earlier, unpublished study by Voegelin on the history of political ideas. Although written a quarter of a century before *The Ecumenic Age,* it logically constitutes a sequel to Voegelin's recent work, showing in detail the implications of his philosophy of consciousness, society, and history for modern political thought and life. *From Enlightenment to Revolution* also demonstrates the impressive continuity of Voegelin's intellectual development. In the analysis that follows, I shall concentrate principally upon these two recently published works.

In some respects, to be sure, my title distorts the character of Voegelin's teaching. Voegelin would resolutely oppose the view that he is presenting his own, merely personal or "subjective," framework for political "evaluation," both because he deplores idiosyncrasy in philosophy and because he considers the "language of 'values' to be the *caput mortuum* [death's head] of a bygone era of methodology." Nonetheless, he would also concede that we must use that language "if we want to make ourselves understood," and he is aware that, following several centuries of "deformation," any attempt to recover the "perennial philosophy" in our time must itself initially appear idiosyncratic.[3]

Voegelin evaluates political ideas and regimes on the basis of an understanding of the human condition gained from a comparative study of symbols concerning spiritual order and disorder which have appeared over time. Indeed, history may be characterized as a "trail of equivalent symbols in time and space" left by the "process" or "moving presence" of reality.[4]

If the meaning of a symbol is to be grasped, great care must be taken not to interpret it literally. To interpret the language of Plato or Aristotle, or of the *Iliad,* or of the Bible

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phers, ed. K. R. Minogue and A. de Crespigny (New York: Dodd, 1975), p. 111.

[3] Eric Voegelin, "Equivalences of Experience and Symbolization in History," in *Eternità e storia* (Florence: a cura dell'Istituto Accademico de Roma, 1970), pp. 215–34, at 215. Voegelin discusses the problem of so-called "value relativism" in an earlier work, where he said in part: "In order to degrade the politics of Plato, Aristotle, or St. Thomas to the rank of 'values' among others, a conscientious scholar would first have to show that their claim to be science was unfounded. And that attempt is self-defeating. By the time the would-be critic has penetrated the meaning of metaphysics with sufficient thoroughness to make his criticism weighty, he will have become a metaphysician himself." Eric Voegelin, *The New Science of Politics* (Chicago: University of Chicago Press, 1952), p. 20.

[4] Voegelin, "Equivalences," p. 233.

literally is to dogmatize and deform it, for symbols are indices pointing to motivating experiences in the souls of the people who articulate them. We must constantly seek to relate symbols meant to illumine political reality to the experiences that gave rise to them. If the symbols are detached from their experiential context, then we shall witness the degeneration of philosophy into the dogmatic formalism of the various "schools," whether neo-Platonic, Stoic, or Scholastic, with their desiccated disquisitions on essences, substances, accidents, etc. From there to the rejection of philosophy itself by those modern intellectuals disenchanted with the detached symbols, seen in their "literal" opaqueness "from the outside" and therefore to be regarded as "irrational,"[5] is but a short step.

As previously noted, Voegelin long ago discovered that "the most basic philosophical problem is the nature of reality."[6] Unfortunately, this is precisely the problem which leading modern "philosophers" tend to neglect, or to assume to have been resolved in some "common sense" fashion, or to be "senseless" and "unverifiable." If, however, one attempts, as Marx did, to abolish the "problem of reality," one also abolishes philosophy.

A philosophy worthy of the name—i.e., one whose symbols are recognizably equivalent to other symbols which have noetically illumined the nature of reality—will acknowledge that reality is not a "disorderly flux of events" but is an intelligible process flowing through human beings. Inasmuch as philosophers are situated within the process rather than at some Archimedian point outside of it, they cannot—in any massive "gnostic" sense—discern its meaning as a whole. The answers to the basic questions of human existence remain shrouded in mystery. Nonetheless, philosophers are also aware that they possess the faculty of *nous* through which the mystery of reality as process becomes luminous to itself *qua* mystery. Philosophy is a knowing and a not-knowing, a knowledge of ignorance and a *cognito fidei*, or knowledge based on faith, in the sense of Hebrews 11:1-3. Faith is the existential virtue of the soul open to the nonmetric, invisible ground: it is the "evidence of things unseen." It is fallacious to apply to unseen nonmetric reality the same tests of validity one applies in the investigation of the phenomenal world.

Voegelin on the "Between" of Human Life

Fundamental to Voegelin's political philosophy is a symbol derived from Plato: that of the "Between" *(metaxy)* of human life. The "human realm of being" is a part, not the whole of reality. To employ a spatial metaphor, the *metaxy* is a field which lies between the "poles" of the "noetic height" and "apeirontic [boundless] depth." These poles establish the "limits set to the philosopher's exploration of reality," and the philosopher "cannot transcend these limits but has to move in the In-Between. . . ."[7] Voegelin writes that Plato "was so acutely aware of man's consubstantiality but nonidentity with divine reality that he developed a special symbol for man's experience of his intermediate status "between" the human and divine: He called the consciousness of this status the *metaxy*, the In-Between of existence." And Voegelin goes on to observe that the "In-Between of existence is not an empty space between two static entities, but the meeting-ground of the human and divine and a consciousness of their distinction and interpenetration."[8] He concludes that "the philosopher is not allowed to settle down on the positive pole of the existential tension; only the tension in its polarity of real and non-real is the full truth of reality."[9]

In a discussion of the early Greek philosopher Anaximander's symbolization of reality, Voegelin speaks of the *metaxy* in connection with two modes of reality, i.e., (1) "reality in existence," comprising the realm of all created things, including human beings in their mortality, and (2) "reality in non-existence," or non-created reality out of which all existing things emerge, which Anaximander designated by the term *apeiron*—the Infinite, or the Boundless, or the Limitless, or the "timeless *archē* of things." "Reality was experienced by Anaximander . . . as a cosmic process in which things emerge from, and disappear into, the non-existence of the Apeiron." Reality in the mode of existence, therefore, is experienced as "immersed in reality in the mode of non-existence and, inversely, non-existence reaches into existence. The process has the character of an In-Between reality, governed by the tension of life and death."[10]

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[5] Voegelin, *From Enlightenment to Revolution*, p. 21.

[6] Ibid., p. 257.

[7] Eric Voegelin, *Order and History*, Vol. 4: *The Ecumenic Age* (Baton Rouge, La.: Louisiana State University Press, 1974), p. 11.

[8] Voegelin, "On Hegel—A Study in Sorcery," *Studium Generale*, 24 (1971), 335–68 at 351. This has also appeared in *The Study of Time*, ed. J. Fraser et al. (New York: Springer-Verlag, 1972), 1:418–51 at 434. It will also appear in *Order and History*, Vol. 5.

[9] Voegelin, "On Hegel—A Study in Sorcery," *Studium Generale*, p. 360; *The Study of Time*, p. 443.

Human consciousness of existence as occurring in the *metaxy* is engendered by theophanic events, and as such is in historical flux, as we can perceive by following the trail of symbols from the compactness of the old myth to the noetic and pneumatic differentiations of consciousness in Greek philosophy and Israelite-Christian revelation, which differentiations require a new mythical language about the experiences of coming into and passing out of existence.

Theophanic events, such as the vision of the *Agathon* by the Platonic Socrates, of the Burning Bush by Moses, and of the Resurrected Christ by Paul, allow human beings to become fully conscious of the range of their humanity and of their intermediate status between mortality and immortality. The *daimonios aner,* or spiritually mature man, is conscious of participating in a process which, in a manner which must remain a mystery, will result in his immortalization *(athanatizein);* as the mortal-immortal, he will strive to attune himself to the highest that is within him, the divine spark. But he will not lose his spiritual "balance" and will be aware that here and now he continues to exist in the cosmos and cannot bestow grace upon himself or achieve immortality within time.

The reader will doubtless be aware of a seeming ambiguity in Voegelin's language: the *metaxy* is portrayed both *as* reality in flux and as the *consciousness* of reality in flux. The "ambiguity" is not attributable to sloppiness of language, however, but is inherent in reality itself. For human beings experience reality only through consciousness; at the same time they are aware of participating in a reality which is greater than consciousness. Consciousness is not the whole, but a process within the whole. There is, then, a truth *of* reality as a process in the Between moving beyond itself which is emphatically to be distinguished from a truth *about* "reality" conceived of as an object "external" to a (self-contained) consciousness.

The reality of human existence (or political reality in the full sense) is *metaleptic;* it can be experienced only through participation from within; it is not a "something," apart from the consciously participating human being, about which propositions can be advanced. Adequate symbols in political philosophy portray the participatory or metaleptic character of reality and are supple and fluid, moving with the flux of reality itself both as to range and depth; inadequate or deformed language about political reality expresses itself in doctrines and propositions and is based upon the eclipse of part of the experiential field. The "clarity" of deformed political language is bought at the terrible price of the simplification, reduction, and reification of human experience. It is, of course, possible to offer the objection that closed existence on the basis of reduction and simplification of experience is preferable to open existence in response to the pull of the divine ground as manifested in the theophanic event. Voegelin's answer to that kind of objection is on principle the same as Plato's in Philebus 16–17, or the prophet Jeremiah's in Jeremiah 45. For life is not a given, and the psyche is a battleground between life and death. The basis for the "preference" for the divine pull of openness over the demonic pull of closure—for theophany over egophany—is the love of life. If, of course, one prefers death in the secure, closed prison of world-immanent existence to potential immortality through faith and trust in the divine presence in the *metaxy,* then there is not much that a philosopher can do about it.

Another obvious objection to Voegelin's teaching is that not everybody experiences the theophanic event and/or recognizes its authority, and that therefore it may be only a private, subjective opinion, or simply "one man's idea about reality."[11] Voegelin's answer is that, while the "structure of reality is not there to be seen by everybody under all personal, social, and historical circumstances," and indeed the "face of reality" becomes visible only to the psyche which is open to the divine ground, nonetheless, the differentiated, noetic experience of reality by the open-souled philosopher is representative for all human beings. The experience is universally representative because human beings possess a "something" that can "respond to a theophany and engage in the quest of the ground." This "something" was called the *diamon* (or spiritual element) by Plato and the *theion* (divine particle) by Aristotle.[12] Every human being is in principle capable of "seeing" the "face" of cognitively structured reality *if* he or she will open the psyche and meditatively reenact the experience of the transcendent ground of Being which is the intelligible source of order and structure in the world. *Without the noetic theophany of the philosophers*—which, although a discovery of the few, was for everybody, and everybody is capable of seeing why this is so—*there could be no cognitively structured reality at all,* for no "systematic exploration of structure in reality is possible unless the world is intelligible," and the world is intelligible only in relation to the one divine ground experienced in the theophanic event that is the divine *nous.*[13]

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[10] *The Ecumenic Age,* pp. 174–75.

[11] *The Ecumenic Age,* p. 186.

[12] Ibid., p. 237.

But why, a critic might continue, should one be bound by the language of past theophanic events, and above all, by the "parochial" vocabulary of the Greek, Judaic, and Christian philosophers, even if one accepts the proposition that no experience of the one divine ground equals no cognitively structured, intelligible reality? Why not leave us completely free to invent our own novel vocabulary? Voegelin's answer is that *there is no abstract language of political evaluation;* there is only the "concrete language created in the articulation of the event."[14] Consciousness itself is "not an abstract entity facing an abstract reality . . . but the consciousness of a concrete man, living in a concrete society, and moving within its historically concrete modes of experience and symbolization."[15] It is fallacious to attempt to abolish "the historical process of consciousness"; the attempt can lead and has led only to deforming both our symbols and our humanity. Human existence has a time dimension. It is ultimately a mystery as to why only in two tiny corners of the world—Israel and Hellas—at a given epoch of time the fully differentiated noetic and pneumatic theophanies occurred. ("How odd of God/To choose the Jews." How odd, indeed, by any rationalistic secularizing account.) But it is *only* with these *concrete* events (of revelation and philosophy) that the one transcendent-divine ground, which is the basis of any cognitively intelligible structure of reality, came into view. Ineluctably, we remain heirs to this classical and Judeo-Christian discovery and symbolic articulation of the world-transcendent divine ground, and today we must seek to move within the orbit of that discovery and articulation if we wish to make sense instead of nonsense out of the political world.

Although human beings exist in the Between, in openness of consciousness they can experience the presence of the divine both as mediated through the "experience of the existence and intelligible structure of things in the cosmos" (which Voegelin calls the Beginning) and as immediately experienced in the "movements of the soul" (which he designates as the Beyond). These two modes of the divine presence in the Between require two different types of language. The Beginning, or the presence of divine reality "mediated by the existence and order of things in the cosmos" requires the language of myth, above all that of the myth of creation; while the Beyond, or the "immediate presence" of the divine in the woul, requires the "revelatory" language of consciousness. "This is the language of seeking, searching, and questioning, of ignorance and knowledge concerning the divine ground, of futility, absurdity, anxiety, and alienation of existence, or being moved to seek and question, of being drawn toward the ground, or turning around, of return, illumination, and rebirth."[16]

Voegelin's study of the trail of symbols that is history reveals man to be the creature capable of asking the Question (with a capital "Q"): "Why should the cosmos exist at all, if man can do no better than live in it as though he were not of it, in order to make his escape from the prison through death?" This, he continues, is the "critical question that brings the mystery of reality into full view: There is a cosmos in which man participates by his existence; man is endowed with cognitive consciousness of the reality in which he is a partner; consciousness differentiates in a process called history; and in the process of history man discovers reality to be engaged in a movement toward the Beyond of its present structure."[17]

Because of the ineradicable uncertainty and ambiguity permeating their existence, and because of the fragility and vulnerability of that condition, human beings are tempted to deform themselves and construct a closed system in whose prison they might find the substitute security lacking in a life open to the tension characteristic of existence in the Between. Particularly since the differentiations of consciousness brought on by Greek philosophy and the Judaic-Christian revelation—both of which dissolved the compactness of the cosmological myth—the temptation to engage in Gnostic system-building has been extremely powerful. The differentiation of consciousness through philosophy and revelation—or what Henri Bergson called the "opening of the soul"—has decisively increased the range of human possibilities both for good and for evil. For human beings are left with the freedom to use their new-found openness to the pull of the eschatological Beyond either in "forming" or in "deforming" their humanity. Specifically, in thinking that is inclined to Gnosticism there is the danger that man may lose his existential "balance" because of his exclusive concentration on the pull of the eschatological Beyond to the neglect of his anchorage in the cosmological Beginning. "For a Gnostic thinker must be able to forget that the cosmos does not emerge from consciousness, but that a man's consciousness emerges from the cosmos." Particularly after the ascendency of Christianity, with its strong eschatological emphasis, Voegelin has found the danger of derailment a serious one.[18] Voegelin's interpretation of Christianity, especially since the publication of volume 4 of *Order and History*, has become the subject of considerable controversy. We shall briefly return to this subject in the conclusion of this article.

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[13] *The Ecumenic Age.* Voegelin is here restating the arguments against an "infinite regress" in Thomas Aquinas (*Summa Theologica,* I, Q. 2, Article 3). Without a "first cause," motion could not be explained, without a Beginning or Source *(Archē),* creaturely existence could not be explained, without an independent Being, dependent beings could not be explained, and without a final Cause, efficient causes could not be explained. The intractability of mythical speculation for scientific explanation is due precisely to the inability of mythical speculation to differentiate the transcendent Ground from intra-cosmic existence.

[14] Ibid., p. 39.

[15] Ibid., p. 75.

[16] *The Ecumenic Age,* pp. 17–18.

[17] Ibid., p. 19. cf. Ibid., p. 320: "The Question is not just any question but the quest concerning the mysterious ground of all being."

Human existence, then, for Voegelin is part of reality, and human beings who live in the "Between" of finite consciousness have the duty of "noetically exploring the structure of reality as far as it is intelligible" and of "spiritually coping with the insight into its movement from the divine Beginning to the divine Beyond of its structure."[19]

"Theophanic" Reality versus "Egophanic" Delusion

Mention of the "structure" of reality brings us close to the heart of Voegelin's framework for political evaluation. However, his position is stated with such philosophical subtlety and profundity that if we do not attend carefully to what he says we shall mistake him for the propagator of yet another "ism" or dogmatic system that promises Gnostic redemption from the perils of this mortal life. Nowhere is his intention to walk the tightrope over the twin abysses of fanatical certitude and dogmatic skepticism in order to recapture the zetetic spirit of the *philosophia perennis* more clear than in his essay "Equivalences of Experience and Symbolization in History."

Although reality has a "structure" with whose divine ground the open-souled person *seeks* attunement, human existence itself is a condition of flux and tension between truth and the "deformation" of reality. Therefore, "ultimate doctrines, systems, and values" are delusions "engendered by deformed existence." The "constants" which we discover to emerge in the time dimension of existence are not dogmas or a catalogue of disembodied propositions, but tensional symbols which illumine the character of existence as occurring between imperfection and perfection, mortality and immortality, order and disorder, attunement and revolt, sense and senselessness, truth and untruth. "If we split these pairs of symbols," observes Voegelin, "and hypostatize the poles of the tension as independent entitles, we destroy the reality of existence as it has been experienced by the creators of the tensional symbols." We then fall prey either to hedonistic apoliticism or to the "murderous" possession of ideological "truth."[20] Dream life then replaces wake life, in the language of Heraclitus and Plato.

To recapitulate, Voegelin teaches—and when I say "teaches," I mean much more than mere advocacy, for he offers voluminous documentation for his teaching, drawing on numerous disciplines and his enormous interpretive and linguistic gifts—that the language of politics may be evaluated in terms of its "correspondence" with "reality," provided that reality be conceived of not as an "object" external to the consciousness of the investigator but rather as a process within which man is situated and which he seeks to illumine from within. Political reality, or the "human realm of being" *(Seinsbereich Mensch)* is not the whole but a process within the whole. The human realm of being may best be characterized by Plato's term, the *metaxy*. In the "Between" of human life, man experiences himself in tension between the poles of openness and closure, attunement and revolt, the divine height and the demonic depth. As the process of reality moves through man and he through it, man questions the origin and the end of the structure which "contains" the process. The Question regarding the Beginning of the reality in which man participates is "answered" initially by the myth of the cosmos as sacred–i.e., as a creation of the gods and later by the great construction of Genesis I–while the Question regarding the Beyond is addressed by the "saving tale" of the philosopher's myth of the immortality of the soul and the promise of resurrection and redemption proclaimed in the Gospels and the letters of St. Paul. There is always the danger that the tension may collapse and a deformed second reality construct may be substituted for symbols illumining the complexities of the only reality that is. (There can no more be two realities than there can be two universes, save in science fiction.) When the symbols are deformed, so are human beings because, although they do not live by symbols alone, they cannot orient themselves to the process of reality without having recourse to adequate symbols.

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[18] *The Ecumenic Age,* p. 20. Voegelin himself has said, however, in response to the question whether he would call himself a Christian, "I try to be, and also to be a philosopher." (Conversation with the author, April, 1975.)

[19] Ibid., p. 28.

[20] Voegelin, "Equivalences," p. 220.

In Voegelin's teaching, then, there is the perpetual danger that we will yield to the temptation to collapse the tension of metaleptic (participatory) reality in the Between of human life and substitute for it a fallacious "second reality" reflecting our own compulsions and "egophanic" imaginings. Indeed, the contrast between "theophanic" and "egophanic" modes of existence is a leading theme of *The Ecumenic Age.* Theophanic existence affirms God as the measure of human conduct, while egophanic existence proclaims "man" in revolt against God as the measure. By "egophany" Voegelin means the attempt to expand the closed self, or ego, to the point where it usurps the place of God and opens up false standards or norms for action in the world. (Literally, "egophany"–a word coined by Voegelin–means "manifestation of the ego," while "theophany" means "manifestation of the divine.") The person who is conscious of his existence in the *metaxy,* which is the "site" for the encounter between man and God in the concrete consciousness of individual men, responds and seeks to attune his life by the "theophanic events of differentiating consciousness." Thus, the symbols engendered by a Plato, a Jeremiah, or a Paul, reflect the tension of existence in the *metaxy* where there occurs the encounter with the divine, although such an encounter does not abolish the character of existence as a tension between contrary poles. Rather, the theophanic event reveals reality as a process moving beyond itself in a manner, which, while it remains a mystery, inspires, through the very act of participation itself, trust that the process will have a good end. For the "experience of reality" has a "built-in bias toward more reality."[21]

Summary of the Principles of Political Evaluation Adumbrated in Volume Four of *Order and History*

Before proceeding to discuss Voegelin's application of his principles of political evaluation to modern developments, let me attempt to summarize those principles as they are expressed in his extremely important work, *The Ecumenic Age;* the reader should bear in mind that no summary can conceivably do justice to the richness of the argument in a major philosophical undertaking, and he or she is, accordingly, urged to read the volume in its entirety.

1. There is only one reality, whose structure as it appears to human beings is that of the *metaxy,* or the tensional field of forces.[22]

2. The divine-human encounter occurs in the *metaxy* in the form of the theophanic event, such as the manifestation of the divine *nous* in Plato's dialogues, or the revelation of the hidden God to Moses in the Burning Bush (Exodus 3), or the revelation of God in Christ to the disciples and St. Paul. These are "noetic" and "pneumatic" theophanies respectively.[23]

3. The theophanic events mark a differentiation in the consciousness of the structure of reality, in that new symbols are engendered, which are more adequate than those of the myth, for expressing the truth of the "paradox of reality": i.e., that reality exhibits a structure which is moving beyond itself. Thus, while the divine reality is one, its presence is expressed in the two modes of the Beyond (philosophy and revelation) and the Beginning (the myth of the visible cosmos as sacred in origin).[24]

4. The symbol "man" indicates the being within Being capable of consciously participating in reality as process and of articulating its structure.[25]

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[21] Voegelin, *Ecumenic Age,* p. 271.

[22] Ibid., p. 255 and *passim.*

[23] Ibid., pp. 9–16, 242–44, 258–59.

[24] Ibid., pp. 17, 253, 314–16.

[25] Ibid., pp. 56, 202, 234–38.

5. As he acts in the world, man has the duty of self-critically exploring the structure of reality and of maintaining his intellectual and spiritual "balance" in the realm of the "Between"; in particular, he must not fallaciously attempt to substitute a second reality based on the delusion that he can abolish the structure and its tension. The source of the duty is the encounter with the divine presence in the theophanic event.[26]

6. Through a complicated process, proceeding through various stages, the order of existence has been inverted in the language and practice of modern political thought. Instead of the sequence man-society-history, found in the classical and Judeo-Christian symbolization of existence, we have the inversion history-society-"man" in the various ideological constructions. Specifically, the process proceeds from:

a. the "deformation" of noetic and pneumatic (or philosophical and revelatory) symbols, in the misguided attempt to protect them from compromise or corruption, by wrenching them from their experiential contexts and reifying them into dogmas or propositions, instead of treating the philosophical or scriptural text as a unity along with the theophanic motivating experience that produced it, to

b. the "egophanic revolt," which attacked not only the "deformed" symbols but also the theophanic experiences that engendered them, thereby enabling the ideological perpetrator of the revolt to pretend that they had abolished the reality of the *metaxy,* substituting for it a tensionless, purely immanent, "reality" governed by some alchemic law of development manipulable by "man" and his expanded innerworldly consciousness.[27]

7. If, as contemporary students of man, society, and history, we are to have any hope of extricating ourselves from the "grotesque intellectual mess," which after two millennia of deformation of symbols and, in the modern period, of the egophanic revolt, continues to obscure and eclipse the structure of reality, we must throw off the intellectual and spiritual encumbrances which weigh so heavily upon us and reorient ourselves to reality as an "open field of theophany." Voegelin's own work is conceived of as a (preparatory and inevitably incomplete) attempt at "open participation in the process of both history and the Whole." We cannot begin to accomplish such a *metanoia,* however, until we reject the various "stop-history" systems put forward by ideologists in the modern period who are closed to the insights of myth, philosophy, and revelation. Only a recovery of the experiential basis of theophanic experience through a renewed philosophy of the consciousness, as distinguished from a mere reassertion of past symbols as "doctrines," can succeed in reversing the centuries-old trend toward egophany. Without such a philosophy of consciousness, the egophanic ideologues will have no difficulty in retaining their power and influence. With it, there is a chance, over many decades, of our regaining the full measure of our humanity.[28]

From Enlightenment to Revolution: **Voegelin on Inverted Reality**

While *Order and History* is Voegelin's *magnum opus,* the implications of his political philosophy for the evaluation of modern politics are nowhere so clearly spelled out as in *From Enlightenment to Revolution,* a volume which Voegelin was persuaded only recently to publish by another distinguished political theorist, John Hallowell of Duke University. As Hallowell explains in his introduction, *From Enlightenment to Revolution* "consists of a portion of an unpublished history of political ideas which Eric Voegelin wrote in the nineteen forties and early fifties." Voegelin's "reluctance" to publish the work at the time was caused by his growing conviction that one cannot study the history of political ideas as if it is an "ongoing argument about commonly perceived problems of social order." For the ideas themselves are rooted in the experiences of participation in reality. If the motivating experiences are themselves distorted by passions—above all the *libido dominandi*—which deflect the psyche from the awareness of its anchorage in the reality of the *metaxy,* then the "reason," cut loose from its moorings in reality, no matter how technically brilliant or logically airtight, can only articulate symbols which destroy the condition of man in society and history. Voegelin, then, came to reject the widely held view that the history of political ideas is a "conversation" or rational debate about common themes, because such an interpretation "assumes a continuity of argument and a universal community of discourse which in fact does not exist."[29]

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[26] Voegelin, *Ecumenic Age,* pp. 11–16, 237–38.

[27] Ibid., pp. 20–57, 260–71.

[28] Voegelin, *Ecumenic Age,* pp. 313–16, 330–35.

Voegelin's willingness to release his study of modern political ideas from the Enlightenment to Marx may be attributed in part to the fact that in the intervening years he has worked out a philosophy of the consciousness and a philosophy of history that form the basis of his interpretation of modern political ideas. However, a careful reading of this work will show that his basic philosophical orientation was already present in this earlier period, although not in fully elaborated form.

Without vainly attempting to recapitulate the entire amount of *From Enlightenment to Revolution,* let me give some examples of the way in which Voegelin has applied the principles of evaluation articulated in his philosophy to the politics of the modern period.

While Voegelin is no apologist for the medieval ecclesiastical polity and while he sharply castigates the Church for its resistance to free inquiry and to the autonomous development of modern science, he has concluded that what has come to be called the "secularization" process took place in the modern West in a manner which has been exceedingly unfortunate for the life of the spirit. The relegation of the Church, which in the medieval order had undertaken to represent the spiritual concerns of Christians, to the private sphere and the consequent assignment to the newly sovereign "secular" political institutions of a monopoly of society's public concerns, "left the field open for a respiritualization of the public sphere from other sources," such as the various ideologies, including nationalism, liberal and socialist versions of "economism," various biological and psychological reductionist doctrines, and a closed collectivist form of "humanitarian tribalism."[30] In the course of this process of "de-divinization" of the world and its reductionist "respiritualization," the classical and Christian understanding of man was destroyed and replaced by an enormously constricted image of "man." Man is no longer interpreted as the Aristotelian *spoudaios* or the Judaic-Christian creature bearing the *imago dei.* He becomes "man" in quotation marks, an inventive, featherless biped who can get along perfectly well without bothersome experiences of transcendence and the pull of the divine Beyond.

During the long and complicated secularization process, the symbolic language of Christianity, drawn from both Hebrew and Hellenistic sources, loses its "transparency for transcendental reality." The Christian symbols of the Fall, Redemption, Resurrection, and the rest are taken as dogmas and are viewed "in a 'literal,' disenchanted opaqueness from the outside." When interpreted in this way—as if they are propositions referring to the world-immanent reality rather than mythopoeic symbols indicating the character of human participation in the *metaxy,* they are judged to be "superstitious," irrational, and in conflict with logic, biology, etc.[31]

Thus, Voegelin argues, the secularization process, instead of deserving to be hailed as a forward step in the history of humankind, should be seen as a tragic story of experiential contraction, symbolic impoverishment, and the diminution of our shared consciousness of what had been known to be man. Voegelin's perception of modernity as a waste land thus approximates that of T. S. Eliot, whose poetry he quotes frequently.

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[29] John H. Hallowell, Introduction to Eric Voegelin, *From Enlightenment to Revolution* (Durham, N.C.: Duke University Press, 1975), p. vi. I have myself expressed the view that the history of political thought is a "conversation of many voices," most recently in my chapter in Nelson Polsby and Fred Greenstein, *Handbook of Political Science* (Reading, Mass.: Addison-Wesley, 1975), I, 229 ff. One problem is to distinguish between "error and the person who errs," as John XXIII expressed it. Voegelin frequently seems to suggest that not only the ideas of Hegel, Marx, and the rest of the "gnostics" are erroneous, but that the thinkers themselves are spiritually diseased as persons. I must confess that I still find it difficult to accept this latter judgment and wonder if it is necessary to condemn these thinkers as whole persons in the way, again, that Voegelin *appears* to argue. Hannah Arendt's remark that there is an "abyss" between facile ideas and brutal ideas needs to be recalled here. And yet, there is an obviously powerful, if unintended, effect of gnostic or messianic ideas on the climate in which a Hitler or a Stalin lives and works. Voegelin's point is that they do not emerge in an intellectual vacuum. Thinkers presumably have an obligation to weigh the probable consequences of their words on future generations of some not so philosophical human beings. However, we today can say all of this about Condorcet, Helvétius, Hegel, Marx and the rest from the advantage of hindsight: of having witnessed the horrors of twentiety century totalitarianism.

The most that I can say at present is that in *From Enlightenment to Revolution* Voegelin has opened up an enormously difficult and important problem of historical interpretation which should concern political scientists in general and all of us who write about the history of political thought in particular much more than it has.

[30] Voegelin, *From Enlightenment to Revolution,* pp. 20, 28.

[31] Ibid., p. 21.

The "progressive" thinkers of the French Enlightenment, such as Voltaire, Diderot, and Turgot, are in particular targets of Voegelin's criticism, because—among other reasons—of their rejection of the Aristotelian primacy of the *bios theōretikos* over banausic activity and for their efforts to confine the meaning of virtue to conduct that is "useful" to society.[32] (Diderot, for example, had a pronounced aversion for the "useless contemplator.")

The implications of the contracted image of man in the writings of the *philosophes* were stated in more radically reductionist form by Helvétius in the eighteenth century and by Saint-Simon and Auguste Comte in the nineteenth. These thinkers and others, whom Voegelin analyzes in detail and with the most scrupulous regard for the sources, "have mutilated the idea of man beyond recognition. . . . This reduction of man and his life to the level of utilitarian existence is the symptom of the critical breakdown of Western civilization through the atrophy of the intellectual and spiritual substance of man. In the progressive, Polisitivist movement since the middle of the eighteenth century . . . the term man no longer designates the mature man of the humanist and Christian tradition, but only the crippled, utilitarian fragment."[33]

Throughout this explosive work, Voegelin is concerned to show that the entire vocabulary of modern Western politics—insofar as it does not continue to be influenced by residues of the classical and Judeo-Christian modes of thought—has been corrupted. Symbols such as "reality," "man," "reason," and "history" no longer mean what they meant prior to their having been "deformed." Thus, after several centuries of symbolic degradation and inversion, Marx is able to reject out of hand the fundamental philosophical question regarding the nature of reality by simply referring to what has by then "in common parlance" come to be called reality.[34] And in the "fragmentary anthropologies" of the moderns, the "normal man" is taken to be the immanentist power-seeker of Hobbes or the pleasure-pain mechanism of the utilitarians, or the "megalomaniac intellectual" of Condorcet and Comte, or Marx's "socialistic man" who asks no metaphysical questions, or Nietzsche's *Übermensch* who extends grace to himself—in fact anyone except the whole person of the classical and Judeo-Christian teaching. The "spiritually mature" human being of the classical and biblical teaching comes to be regarded as a freak or relic from the past—someone to be psychoanalyzed or otherwise dissected or explained but certainly not someone to be taken as a model for emulation and a standard for evaluation.[35]

Just as the symbols "reality" and "man" acquired radically changed meanings during the modern process of deculturation, so in like manner did symbols such as "reason," "history," and the "science of politics." Reason, understood in the Platonic-Aristotelean sense as the faculty for self-critically illuminating the "total context" in which humanity's personal existence is situated, is degraded to the level of a faculty of cleverness and technical expertise, as an instrument through which human beings maximize their desires. It becomes the servant of the passions, inverting the Platonic psychology. The closed self, in its inverted existence, builds a prison where it finds a garden. Ultimately, the modern egophanic speculator seeks to freeze history itself, and we witness the rise of what Voegelin calls the "stop-history movements." The stop-history movements regard the latest phase of human evolution as the culminating one, and they reject on principle the possibility that future developments in political life and thought may be dramatically different from the present ones. The modern "progressive," trend-riding intellectuals are closed to the time-dimension of existence and to history as an open field of theophany.[36] They are, in fact, anything but "progressive."

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[32] Voegelin, *From Enlightenment to Revolution,* pp. 27–28.

[33] Ibid., p. 95.

[34] Ibid., p. 258. Voegelin here quotes the judgment of S. Landshut and J. P. Mayer, Introduction to *Marx: Der historische Materialismus* (Leipzig, 1932), I, xxii. Voegelin points out that if Marx had been open to the question regarding the ground of human existence, he would not have produced an ideology which denies that man is situated in an order of being and a cosmic order to which he is subordinated. Had they raised the question, Marx and Engels would have correctly recognized themselves as "predicates" of a divine subject instead of indulging in the illusion that they were Promethean creators of a new reality. Had Marx raised the question of the nature of reality, he would have been led to recognize the "total context" of which he was only a minute part. Instead, Voegelin contends, Marx engaged in "pseudo-logical speculation," and "grandiose ranting," with the result that he and his followers produced a "general intellectual mess" on the basis of illustory second-reality constructions. "And these," Voegelin wryly concludes in his section on Marx, "are the ideas that shake the world!" (Ibid., p. 272). Voegelin's critique of Marx provides philosophical underpinning for Solzhenitsyn's judgment, in his 1974 Stockholm Nobel Prize interview, "Solzhenitsyn Speaks out" (reprinted in *National Review,* 6 June 1975, 606), that it is wishful thinking to draw a dramatic distinction between the "humanism" of Marx and the antihumanism of Lenin and Stalin.

As mentioned in note 29, the difficult problem as I see it, is to reconcile the demands for intellectual perspicacity with those of human charity (to employ a distinction of the late Leo Strauss). Without indulging in sentimentality, we are obliged to be charitable to our fellow human beings in the Between. I cannot imagine that Voegelin would disagree with my last statement.

[35] Voegelin, *From Enlightenment to Revolution,* pp. 69, 97, 132, 178, 258.

Some Concluding Observations

From his study of the widest possible range of evidence—e.g., the comparative study of symbols engendered over the millennia by those who have raised the basic questions about the Why of existence and the nature of reality as a process experienced in the Between of human life—Voegelin concludes that the modern world is now and may be for another century in the throes of a spiritual crisis of unprecedented proportions. The key symbols of our political discourse have become distorted and corrupted with "second reality" connotations. Our political science, under the impact of positivization which has done so much to restrict the range and depth of political inquiry and, indeed, of the concept of the political itself, has largely lost the capacity thoughtfully to raise the "great questions" of political philosophy. It accordingly has been a negligible force in combating the fallacy of the two realities which lies at the root of extremist ideological thinking and in exposing the inversion of the proper sequence of man-society-history in so much of our contemporary political and social analysis, both within and without the academy.

Alfred North Whitehead once observed that "the world never recovers from the shock of a great philosopher." Whether this observation is valid for our time is certainly debatable. We appear largely to have lost the capacity to recognize a philosopher when we see one. That this is so is evident in part from the slight attention (relative to its importance) which Voegelin's work has thus far received in political science circles.

Of course, it is painful to recognize that a drastic revision of one's language, priorities, and evaluations may well be required if what Voegelin says is true. Whether his teaching is valid in general—and which aspects are in need of refinement or revision—is a matter which can be established only after a thoroughgoing debate, *at the philosophical level.* It would be counter to the entire spirit of openness of inquiry in philosophy per se and of Voegelin's work in particular to accept his teaching automatically and uncritically. But it would also be counter to that spirit to reject it automatically and uncritically, and, given the climate of the times, the latter response is far more likely to be encountered than the former. In fact, to accept Voegelin's teaching as a *doctrine* would violate his fundamental premise of the priority of motivating experience over the "dead" letter.

A frequent form of the ritualistic rejection of a philosophical teaching is what Voegelin himself calls "positionism." That is, if one can label a thinker something different from one's own "position," then one is dispensed from the burden of having to think seriously about what that person has to say. Because of his openness to experiences of transcendence, for example, one not infrequently hears Voegelin dismissed as a "confessional" or "sectarian" thinker. And yet, Voegelin's chapter on "The Pauline Vision of the Resurrected" has prompted great concern from many Christians because of his treatment of the problem of the physical Resurrection and the general question of the "historical Jesus."[37] It would be impossible here to deal with all the complexities of the issue, but Voegelin's emphasis upon the Resurrection of Christ as a "vision" which takes place in the consciousness of the disciples and of Paul should not be understood as implying a merely "subjective" or allegorizing interpretation of the event. As with all theophanic events, the Resurrection for Voegelin occurs in and, indeed, engenders, the reality of the Between of differentiating consciousness. It is therefore inappropriate to split the event, which is a unity, into an objective "something" and a subjective awareness of that "something." As Voegelin expressed the matter in principle:

> If the metaleptic [participatory] symbol which is the word of both god and man is hypostatized into a doctrinal Word of God . . . it can impair the sensitivity for the source of truth in

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[36] Voegelin, *From Enlightenment to Revolution,* pp. 72–73.

[37] Voegelin, *The Ecumenic Age,* Ch. 5, pp. 239–71. See the vehement review by Frederick D. Wilhelmsen in *Triumph* (January 1975), pp. 32–35, for an admittedly rather sensationalist manifestation of this concern.

> the flux of divine presence in time which constitutes history. Unless precautions of meditative practice are taken, the doctrinalization of symbols is liable to interrupt the process of experiential reactivation and linguistic renewal. When the symbol separates from its source in the experiential *metaxy,* the Word of God can degenerate into a word of man that one can believe or not.[38]

Voegelin's intention throughout his work is not to advocate a "position," but "to follow empirically the patterns of meaning as they reveal themselves in the self-interpretation of persons and societies in history."[39] He is poles apart from the ideological doctrinaire, who imposes an a priori pattern upon events. Thus, in *From Enlightenment to Revolution,* after two chapters of relentless criticism of the epistemology and "ontology" of Marx, Voegelin credits Marx with having "laid his finger on the sore spot of modern industrial society," viz., "the growth of economic institutions into a power of . . . overwhelming influence on the life of every single man. . . ." Thus, he argues, Marx treated under the heading of "alienation" the problem that "in an industrialized society man is not the master of his economic existence." Voegelin also castigates economics as a science for its failure to follow Marx's pathbreaking analysis of the relationship between man and nature to the point where it would have developed a philosophy of labor.[40]

Eric Voegelin has in many works, but principally in his *magnum opus, Order and History,* presented us with a philosophically profound analysis of the structure of reality in the *metaxy* which is highly relevant to the concrete problem of political evaluation in our time. Although much of the accent of his analysis of modern developments is negative, his teaching is by no means unhopeful, and he recognizes that there are encouraging signs of intellectual and spiritual resistance to the symbolic impoverishment and degradation which has been the dominant tendency of modern politics, and beyond that of the possibility of experiential and linguistic reactivation and renewal. But although he is scarcely without hope, he is modest in his expectations about the impact of his own work on the crisis of our time:

> Nobody can heal the spiritual disorder of an "age." A philosopher can do no more than work himself free from the rubble of idols which, under the name of an "age," threatens to cripple and bury him; and he can hope that the example of his effort will be of help to others who find themselves in the same situation and experience the same desire to gain their humanity under God.[41]

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[38] Voegelin, *The Ecumenic Age,* p. 56.

[39] Ibid., p. 57.

[40] Voegelin, *Enlightenment to Revolution,* pp. 299–300.

[41] Voegelin, "On Hegel–A Study in Sorcery," *Studium Generale,* 24 (1971), 355–68, at 349, *The Study of Time,* p. 432. To be reprinted in *Order and History,* Vol. 5.

The Myth of the Tradition

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Leo Strauss's epic rendition of the history of Western political philosophy has been a principal factor in the establishment and perpetuation of the myth of the tradition or the belief that the conventional series of classic works from Plato to Nietzsche represents the development of modern political ideas and constitutes the core of an inherited pattern of thought which, in turn, provides the basic context for interpreting particular texts. Much of the scholarly commentary on the history of political philosophy has been directed toward a critique of contemporary political thought and action, and the idea of the tradition has served as a vehicle for this historical etiology. In Strauss's argument, the concept of the tradition plays a strategic rhetorical function, but the myth of the tradition in its various forms has become a pervasive regulative assumption in both teaching and research.

Some of the most influential accounts of the history of political philosophy as well as interpretations of particular texts have been informed by a concern with developing a critique of contemporary politics and political thought. This attempt to explain and evaluate the present in terms of its intellectual antecedents has contributed to the entrenchment of the belief that the conventional chronology of classic texts (including at least the works of Plato, Aristotle, St. Augustine, St. Thomas Aquinas, Machiavelli, Hobbes, Locke, Rousseau, Hegel, and Marx) constitutes an actual historical tradition or inherited pattern of thought. It is assumed that these works belong to a distinct genre, that they are the product of a discrete activity with a recognizable temporal career, and that they represent the evolution of western political ideas. This reconstructed tradition has become the principal context for understanding the meaning of past texts, and the significance attributed to a work has become largely a matter of its role in this historical narrative. The argument of Leo Strauss presents a paradigm case of the myth of the tradition.

There is no doubt that Leo Strauss significantly influenced teaching and research in the history of political philosophy, yet although he has been summarized, criticized, eulogized, and elegized, the character of the Straussian enterprise has still remained elusive. This is in part because discussions of his work have been caught between polemics and apologetics and have often focused on such narrow but controversial issues as his attack on modern social science, his iconoclastic interpretations of figures such as Machiavelli and Locke, his defense of natural law, his concern with esoteric writing, and his animadversions on contemporary politics. But it is also in part because it is not easy to characterize the activity in which Strauss is engaged and the criteria appropriate for understanding and judging his arguments. The history of political philosophy is not a well-defined discipline with accepted research programs, and to locate Strauss within this field is of limited value. To understand Strauss requires a comprehensive view of his enterprise and a careful examination of what he has to say about it, but a clarification of Strauss's work may yield some insight into certain other literature usually subsumed under the category of the "history of political philosophy."

Although some of Strauss's critics denigrate the study of the history of political philosophy in general, many of the disputes generated by his work have been interspecific, and, consequently, some of the family resemblances between his arguments and other literature in the field have received little attention. In many respects Strauss's concept of a tradition of political philosophy exemplifies a regulative paradigm discernible in textbooks and other general treatments of the history of political philosophy as well as studies of individual works. My purpose is to present a systematic exposition and analysis of Strauss's argument about, and approach to, the history of political philosophy. However, I also wish to identify and illuminate the structural attributes and governing intentions characteristic of this type of literature.

In recent years, there has been an increasing concern with the "methodology" of the history of ideas and the exegesis of historical texts which carries important implications for research in the history of political philosophy.[1] But relevant criticism in this field should also entail an understanding of the premises underlying this activity. Strauss's work is distinctive in many ways, yet it belongs to a genre which has dominated teaching and research in this area for some time. I do not suggest that Strauss speaks for the field; however, his work is more representative than either his defenders or his critics acknowledge.

I

Strauss maintains that his attempt to understand the works of past political philosophers and the development of political ideas does not derive from a *"pain-loving antiquarianism"* or "intoxicating romanticism" but from a confrontation with what he diagnoses as the crisis of modernity: "We are impelled to do so by the crisis of our time, the crisis of the West."[2] The purpose of the enterprise is essentially therapeutic.

The crisis which Strauss alleges is both intellectual and political and pervades every aspect of contemporary culture. Although the crisis, its symptoms, and its causes tend to blur somewhat in his analysis, this is because he sees the crisis as the product of historical evolution. An understanding of the character of this modern predicament, its sources, and the key to its solution is accessible only through exploration of its origins. Strauss views the crisis as a result of the degeneration of the Western tradition, and, in particular, the tradition of political philosophy. The history of political philosophy must examine the process which led to this decline and regain what has been lost. The task is one of recapitulation and restoration.

Politically, the crisis involves the threat of "communism" and an "Eastern despotism," and Strauss argues that already "some decline of the West has taken place before our eyes," and that the "West's very survival is endangered by the East" in a manner unparalleled since ancient times.[3] But, according to Strauss, the external threat is complemented by an even more serious internal problem: the West has "become uncertain of its purpose."[4] The "decline of liberal democracy into permissive egalitarianism" has endangered its very existence, since "a permissive society which permits its members every sort of non-permissiveness will soon cease to be permissive. It will vanish from the face of the earth."[5] In our time, he argues, "liberalism has abandoned its absolutist basis and is trying to become entirely relativistic."[6] Modern liberalism, which is characterized by a "passionate rejection of all 'absolutes' " including those on which its own legitimacy is based, is actually a "seminary of intolerance" in disguise.[7]

Strauss maintains that the thesis "that we are in the grip of a crisis is hardly in need of proof" and that it is equally evident that the cause of this crisis is "the modern project" and its search for the conquest of nature.[8] He argues that the "inadequacy of the modern project, which has now become a matter of general knowledge and of general concern, compels us to entertain the thought that this new kind of society, our kind of society, must be animated by a spirit other than that which animated it from the beginning."[9] Apart from his roster of "isms" (communism, historicism, relativism, liberalism, positivism, nihilism, etc.), Strauss is not very specific about the agents of the threat to the West, but he insists that the conditions of the age are such that

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[1] See Quentin Skinner, "Meaning and Understanding in the History of Ideas," *History and Theory,* 8 (1969), 3–53; and "Some Problems in the Analysis of Political Thought and Action," *Political Theory,* 2 (August 1974), 277–303. For critical discussions of Skinner's arguments and further analysis of these problems, see Richard Ashcraft, "On the Problem of Methodology and the Nature of Political Theory," *Political Theory,* 3 (February 1975), 5–25; Bhiku Parekh and R. N. Berki, "The History of Political Ideas: A Critique of Q. Skinner's Methodology," *Journal of History of Ideas,* 34 (1973), 163–84; Margaret Leslie, "In Defense of Anachronism," *Political Studies,* 4 (1970), 433–47; J. G. A. Pocock, *Politics, Language, and Time* (New York: Atheneum, 1971); Gordon Schochet, "Quentin Skinner's Method," *Political Theory,* 2 (August 1974), 261–76; Charles Tarlton, "Historicity, Meaning and Revisionism in the Study of Political Thought," *History and Theory,* 12 (1973), 307–28.
Despite the importance of the issues raised, these studies, for the most part, do not explore the intentions of the historian of political theory, and they assume that his endeavor can be comprehended and evaluated as a species of intellectual history. For a discussion of these problems as well as an analysis of other secondary literature on the history of political theory, see John G. Gunnell, *Political Theory: Tradition and Interpretation* (Cambridge: Winthrop, 1978).

[2] Leo Strauss, *The City and Man* (Chicago: Rand McNally, 1964), p. 1.

[3] Leo Strauss, *The City and Man* pp. 2–3.

[4] Ibid., p. 3.

[5] Leo Strauss, "Political Philosophy and the Crisis of Our Time," in *The Post-Behavioral Era,* ed. George J. Graham, Jr. and George W. Carey (New York: David McKay, 1972), p. 242.

[6] Leo Strauss, "Relativism," in *Relativism and the Study of Man,* ed. Helmut Shoeck and James W. Wiggins (Princeton: D. Van Nostrand, 1961), p. 140.

[7] Leo Strauss, *Natural Right and History* (Chicago: University of Chicago Press, 1953), pp. 5–6.

[8] "Political Philosophy and the Crisis of Our Time," pp. 217–18.

> we are now brought face to face with a tyranny which holds out the threat of becoming, thanks to "the conquest of nature" and in particular human nature, what no earlier tyranny ever became: perpetual and universal.... The manifest and deliberate collectivization or coordination of thought is being prepared in a hidden and frequently quite unconscious way by the spread of the teaching that all human thought is collective independently of any human effort to this end, because all thought is historical.[10]

The political crisis, then, is the consequence of a fundamental intellectual crisis. This, he contends, may in turn be attributed to the decline of political philosophy. In the modern age, "what was originally a political philosophy has turned into an ideology," and this is "the core of the contemporary crisis of the West."[11]

In a functional sense, Strauss defines political philosophy as an activity which seeks knowledge for the purpose of changing the present, as far as practicable. Just as philosophy in general is concerned with replacing opinions with knowledge, political philosophy is "the attempt to replace opinion about the nature of political things by knowledge of the nature of political things" which is primarily knowledge about the right or good political order.[12] But Strauss also wishes to locate political philosophy historically and to distinguish it from the wider category of political thought. While the latter, he suggests, is "as old as the human race and political life itself," the former "appeared at a knowable time in the recorded past" and "has been cultivated since its beginnings almost without any interruption until a relatively short time ago. Today, political philosophy is in a state of decay and perhaps of putrefaction, if it has not vanished altogether."[13] As a specific historical phenomenon, political philosophy was "originated by Socrates,"[14] elaborated by Plato and Aristotle, and continued, at least in an attenuated form, until contemporary times.

Although Strauss insists that "Science and History, those two great powers of the modern world, have finally succeeded in destroying the very possibility of political philosophy,"[15] they are as much the product of the tradition of political philosophy, and in particular modern political philosophy which emerged in the sixteenth and seventeenth centuries, as the cause of its demise. Thus we must study the history of political philosophy to understand what has been lost and how it has been lost. Positivism and historicism together create a climate of "unqualified relativism" which has come to characterize "Western thought in general" and which, in its rejection of the possibility of "knowledge of natural right" transcending particular social and historical contexts, "leads to nihilism" or the reluctance and "the inability to take a stand for civilization against cannibalism."[16] Just as political philosophy in its most classic form embodied the purpose of discovering the nature of political things including the nature of the good society, positivism and historicism, which constitute a denial of the validity and possibility of that undertaking, are most characteristically expressed in contemporary social science.

Strauss argues that positivism has been the basic intellectual stance of social science since the nineteenth century. With its identification of science and knowledge with empiricism and its equation of values with "blind preferences," positivism undercuts political philosophy.[17] But "positivism necessarily transforms itself into historicism" which in its various forms constitutes "the spirit of our time" and is by far the most "serious antagonist of political philosophy."[18] Historicism goes beyond positivism by regarding *all* claims to knowledge as relative to particular modes of culturally and historically conditioned experience and incapable of ultimate justification.[19] Historicism spawned a paralysis of thought and action which not only weakened the moral nerve of Europe in 1933 but continues to pervade Western culture and encourages the assumption that all value judgments are equally worthy.

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[9] "Political Philosophy and the Crisis of Our Time," p. 217.

[10] Leo Strauss, *On Tyranny* (Glencoe: Free Press, 1963), pp. 26–27.

[11] "Political Philosophy and the Crisis of Our Time," p. 218; *The City and Man*, p. 2.

[12] Leo Strauss, *What is Political Philosophy?* (Glencoe: Free Press, 1959), pp. 11–12.

[13] Ibid., pp. 12–13, 17.

[14] Leo Strauss, "Introduction," in *History of Political Philosophy*, ed. Leo Strauss and Joseph Cropsey (Chicago: Rand McNally, 1963), p. 2; Leo Strauss, *Socrates and Aristophanes* (New York: Basic Books, 1960), p. 3.

[15] *What is Political Philosophy?*, p. 18.

[16] *Natural Right and History*, pp. 2–3, 5; Leo Strauss, "Social Science and Humanism," in *The State of the Social Sciences*, ed. Leonard D. White (Chicago: University of Chicago Press, 1956), p. 422.

[17] *Natural Right and History*, p. 4.

[18] *What is Political Philosophy?*, pp. 25–26, 57.

[19] *Natural Right and History*, pp. 18–19.

Strauss declines to attribute base motives to contemporary social science, but he views it as a repository of modern ideas which reinforces while it reflects these ideas. Social science, with its positivist and historicist assumptions, is unprepared for either a defense or constructive criticism of liberal democracy, let alone the required "relentless critique of communism."[20] Thus it contributes to the political crisis, and although it directly "fosters not so much nihilism as conformism and philistinism," it is closely tied to both the intellectual and political dimensions of modern nihilism.[21]

For Strauss, political science, which is the direct heir to the tradition of political philosophy, is the existential manifestation of the decline of political philosophy. What Strauss terms the "new science of politics," typified by American political science, may not have caused the "crisis of the modern Western world," but "it is surely contemporary with that crisis."[22] Since Strauss views modern political science as rejecting traditional political philosophy and adopting the ideas of positivism and historicism, he selects it as a major object of attack. He perceives the "new political science" not merely as an academic discipline based on false premises but as an educational institution which "wields very great authority."[23]

Strauss's indictment of modern political science includes the charges that it "rests on a dogmatic atheism," and involves the "consistent denial of a common good." He charges also that it denies that "man has natural ends" or that "there is any essential difference between men and brutes," and accepts a thoroughgoing relativism which "unwittingly contributes to the victory of the gutter." Furthermore, since it is "permissive or liberal" in its implications, it reflects the most dangerous proclivities of democracy" by teaching "the equality of literally all desires."[24] He holds that there is not only a "harmony between the new political science and a particular vision of liberal democracy," but its extreme "democratism" tends to depreciate differences "between liberal democracy and communism."[25] It not only blurs the doctrinal distinctions but treats them as simply two incommensurable historical ideologies and consequently "has nothing to say against those who unhesitatingly prefer surrender, that is, the abandonment of liberal democracy, to war."[26] Thus it contributes to the external threat to the West by weakening the internal foundations of democracy and by refusing to make value judgments and recognize "tyranny" for "what it really is."[27] But Strauss insists that modern political science, despite its perverse tendencies and influence, is more naive than "diabolic" in a Neronic sense. "It is excused by two factors: it does not know that it fiddles, and it does not know that Rome burns."[28]

Since, for Strauss, the explanation of the modern crisis is the decline of political philosophy, any solution to the crisis presupposes an understanding of this decline and knowledge of the original character of political philosophy. He argues that "we are therefore in need of historical studies in order to familiarize ourselves with the whole complexity of the issue."[29] Yet he insists on differentiating his undertaking from histories of political philosophy informed by historicist premises. Strauss contends that "the decay of political philosophy into ideology reveals itself most obviously in the fact that in both research and teaching, political philosophy has been replaced by the history of political philosophy."[30]

Strauss argues that the purpose of research in the history of political philosophy is not to gain knowledge about the past, or the present, as an end in itself. It is an exercise necessary for the diagnosis of modern ills and the restoration of political philosophy in its original form. Although historical knowledge may in some sense be useful to political philosophy in gaining knowledge of political phenomena, "political philosophy is not a historical discipline" and "is fundamentally different from the history of political philosophy itself."[31]

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[20] "Social Science and Humanism," p. 418.

[21] *What is Political Philosophy?*, p. 20.

[22] Leo Strauss, "Epilogue," in *Essays on the Scientific Study of Politics*, ed. Herbert J. Storing (New York: Holt, Rinehart and Winston, 1962), p. 307.

[23] Ibid., p. 307.

[24] Ibid., pp. 322–26.

[25] Leo Strauss, "Epilogue," pp. 319, 326.

[26] Ibid., p. 327.

[27] *On Tyranny*, p. 22.

[28] "Epilogue," p. 327.

[29] *Natural Right and History*, p. 7.

[30] *The City and Man*, pp. 7–8.

[31] *What is Political Philosophy?*, p. 56.

Strauss maintains that while most modern historians accept the premises of nineteenth-century historicism and assume that all political thought has an equal claim on truth, his own history aims, in part, at exposing the error of what amounts to a denial of "the fundamental distinction between philosophic and historical questions."[32] The assumption that all works are merely expressions of their historical period constitutes a rejection of the original conception of political philosophy as a means of discovering universal truths about politics.

The historicist approach to the past is instrumental in the sense that its purpose is "activist" rather than "contemplative" and involves the demand "that each generation reinterpret the past on the basis of its own experience and with a view to its own future" and "ultimate guidance for political life."[33] But Strauss's approach is also instrumental. Although he maintains that the study of the past cannot provide "recipes" for the present, he does allow that "an adequate understanding of the principles as elaborated by the classics may be the indispensable starting point for an adequate analysis, to be achieved by us, of present-day society in its peculiar character, and for the wise application, to be achieved by us, of these principles to our tasks."[34] Strauss's instrumentalism is actually more radical, since he implies that once the task of the history of political philosophy is completed, such histories could in principle be eliminated and replaced by political philosophy itself or the knowledge achieved by a particular philosophy.

Strauss realizes the paradox in his insistence, on the one hand, that political philosophy is a non-historical activity and his contention, on the other hand, that historical studies are required to recover political philosophy. He admits that this inevitable tension makes it difficult to maintain the necessary separation between philosophy and history. However, he insists that to engage in the history of political philosophy simply to prevent "the burial of a great tradition" is something which "is not merely a half-measure but an absurdity: to replace political philosophy by the history of political philosophy means to replace a doctrine which claims to be true by a survey of more or less brilliant errors."[35]

It is not only that the modern crisis demands the history of political philosophy but also that, in a fundamental sense, it makes it feasible by facilitating a break with traditional patterns of thought. The task of achieving adequate knowledge of the past has "been rendered possible by the shaking of all traditions; the crisis of our time may have the accidental advantage of enabling us to understand in an untraditional or fresh manner what was hitherto only understood in a traditional or derivative manner."[36] The tradition becomes accessible at the point of its crisis. Only at such a propitious moment is it possible to detach oneself from the tradition sufficiently to maintain the requisite objectivity to gain "solid knowledge" of the past and "to study the political philosophies as they were understood by their originators."[37]

There are, in Strauss's view, "two opposed ways in which one can study the thought of the past." The variety of historicist interpretations have in common the implicit or explicit denial that it is possible to achieve objective historical knowledge and "understand the thinkers of the past exactly as they understood themselves."[38] They insist on understanding these thinkers in light of their significance for the present and engage in a creative attempt "to understand the thinkers of the past better than those thinkers understood themselves" which always involves "a questionable mixture of interpretation and critique."[39] But Strauss maintains that the goal must be "to understand the thought of the past 'as it really has been,' i.e., to understand it as exactly as possible as it was actually understood by its authors."[40]

Once the ideas of the past have been objectively understood, they can then be submitted to a "philosophic critique concerned exclusively with their truth or falsehood," and it becomes apparent that "what at first sight is merely the result of the demands of historical exactness is actually the result of the demand for a philosophic reexamination of our basic assumptions."[41] By understanding the thinkers of the past exactly, we come to a more clear and self-conscious understanding of ourselves. Modern political philosophy requires the aid of the history of political philosophy, since "it consists to a considerable extent of inherited knowledge whose basis is no longer contemporaneous or immediately accessible." Thus "a special effort is required to transform inherited knowledge into genuine knowledge by revitalizing its original discovery, and to discriminate between the genuine and spurious elements."[42] The goal of the history of political philosophy is "the restoration of political philosophy,"[43] but this restoration must be accompanied by a critical or destructive effort which strips away the distortions of a tradition that has separated the modern age from a true understanding of politics.

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[32] *What is Political Philosophy?*, p. 57.

[33] Ibid., p. 59.

[34] *The City and Man*, p. 11.

[35] Ibid., p. 8.

[36] *The City and Man*, p. 9.

[37] Ibid., p. 9.

[38] *On Tyranny*, p. 24; *What is Political Philosophy?*, p. 67.

[39] *On Tyranny*, p. 24.

[40] Ibid., p. 24.

[41] *What is Political Philosophy?*, p. 66; Leo Strauss, "On a New Interpretation of Plato's Political Philosophy," *Social Research*, 13 (1946), 332.

According to Strauss, the "great tradition" of political philosophy may be broken down into the classical and modern periods. In his view, this is not an arbitrary division but a distinction grounded in a deliberate transformation of the classical teaching by modern philosophers. "Modern political philosophy came into being through the conscious break with the principles established by Socrates," and it can only be understood as at once a derivation from and a rejection of those principles.[44] To comprehend the spirit of the modern age, one must understand that it rests on the suppositions of modern political philosophy, but the latter has in turn evolved from classical political philosophy, despite its divergence from it. Classical political philosophy becomes the essential starting point for understanding the entire tradition.

A crucial element in Strauss's argument is his thesis that "classical political philosophy is the true science of political things," and he maintains that the "essential character of all political situations was grasped by the old political science."[45] Strauss maintains that when starting from a historicist perspective "the obvious possibility is overlooked that the situation to which one particular doctrine is related, is particularly favorable to the discovery of *the* truth" or that "a political philosophy which emerged many centuries ago is *the* true political philosophy."[46] The aim of the history of political philosophy must be to penetrate the tradition, to peel away the accretions, and to reach a point where classical political philosophy can be viewed directly, just as it viewed political things directly, rather than "through the lenses of modern political philosophy and its various successors."[47] The modern break with classical political philosophy marks the beginning of the decline of political philosophy, and thus

> we have to go back to the point where the destruction of political philosophy began, to the beginnings of modern political philosophy, when modern philosophy still had to fight against the older political philosophy, classical political philosophy, the political philosophy originated by Socrates and elaborated above all by Aristotle. At the same time, the quarrel of the ancients and moderns took place.... It was fundamentally a quarrel between modern philosophy, or science, and the older philosophy, or science.... Our task is to reawaken that quarrel, now that the modern answer has been given the opportunity to reveal its virtues and to do its worst to the old answer for more than three centuries.[48]

Strauss wishes to illuminate and reopen this old quarrel between the ancients and the moderns for a very practical purpose. Since the modern "project" was originated by modern political philosophy, to uncover the hidden meaning of modern political philosophy and destroy its creditability is to undercut the foundations of modern liberalism, communism, and allied doctrines which are, in his view, at the center of the crisis of the West.[49] By demonstrating the defective and derivative character of modern political philosophy and comparing it with the indefectible and primary character of classical political philosophy, he attempts to call into question the claims of modernity regarding both its understanding and practice of politics.

Strauss maintains that "our political ideas have the particular character that they cannot be clarified fully except by means of historical studies, whereas the political ideas of the past could be clarified perfectly without any recourse to their history."[50]

> Classical political philosophy is non-traditional, because it belongs to the fertile moment when all political traditions were shaken, and there

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[42] *What is Political Philosophy?*, p. 77.

[43] "Political Philosophy and the Crisis of Our Time," p. 218.

[44] "Introduction," p. 2.

[45] *The City and Man*, p. 10; "Epilogue," p. 313.

[46] *What is Political Philosophy?*, pp. 68, 69.

[47] *The City and Man*, p. 9.

[48] "Political Philosophy and the Crisis of Our Time," pp. 217–18.

[49] *What is Political Philosophy?*, p. 172.

[50] Ibid., p. 74.

> was not yet in existence a tradition of political philosophy. In all later epochs, the philosophers' study of political things was mediated by a tradition of political philosophy which acted like a screen between the philosopher and political things, regardless of whether the individual philosopher cherished or rejected that tradition. From this it follows that the classical political philosophers see the political things with a freshness and directness which never have been equalled.[51]

All subsequent political thought is distinguished by an increasing tendency toward the "abstract" and away from the "concrete" and "simple and primary issues."[52] This is particularly manifest in modern social science which, standing at the end of the tradition, "abstracts from essential elements of social reality" and is removed from the terms of "common-sense thinking" or the point of view and language of the citizen.[53]

Classical political philosophy achieved an unmediated grasp of political phenomena, since it "acquired the fundamental concepts of political philosophy by starting from political phenomena as they present themselves to 'the natural consciousness,' which is pre-philosophic consciousness."[54] The meaning of these seminal concepts was subsequently modified in the course of the tradition until there was no longer a correspondence between the concepts and political reality. But the phenomena themselves were in a sense also transformed, because after classical Greece, philosophy became part of political life. Only at that one crucial moment in history did a non-traditional mode of thought confront a philosophically uncontaminated political life. And only now, when the tradition has run dry and Western culture falls into crisis, does the opportunity present itself to commence an intellectual journey, through the medium of the history of political philosophy, for the purpose of regaining that original understanding of political phenomena.

Strauss seems to suggest, however, that the essential nature of political things has remained constant despite transformations in historical form, because the nature of man and human relations are unchanging. Yet original political concepts have become so modified and tradition-laden that their original meaning can only be regained by radical re-thinking. "These concepts can ... be understood, and their validity can be checked, by direct reference to phenomena as they are accessible to 'natural consciousness,' "[55] but the modern consciousness is not natural but traditional. Strauss does not elaborate on this "phenomenological" epistemology, but he insists that the means of achieving a "natural consciousness" of "political things" cannot be created by a mere act of will in the present. It can be won only by a historical penetration of the tradition which brings us back to Aristotle's *Politics* and classical political science's virginal encounter with political phenomena.[56]

Most of Strauss's particular studies of the writings of political philosophers, as well as those of many of his students and epigones, are devoted to an explication of, or are approached in terms of, his general vision of the structure and meaning of the tradition of political philosophy. Overall, the vision is one of progressive deterioration, but the fundamental division is between the ancient and modern periods, since all modern political philosophies "have a fundamental principle in common" which is the "rejection of the classical scheme as unrealistic."[57] Strauss contends that this rejection and transformation marked the beginning of cumulative error in the tradition. Just as Socrates was the founder of the classical portion of the tradition, Strauss maintains that "the founder of modern political philosophy is Machiavelli."[58]

Strauss insists that modern political science, which is blind to the difference between tyranny and other regimes, has its origin in Machiavelli's work which is characterized by the same indifference, and to understand the assumptions of modern political science, it is necessary to understand the character of the change in the tradition that was consciously brought about by Machiavelli.[59] Machiavelli

> tried to effect, and he did effect, a break with the whole tradition of political philosophy. He compared his achievement to that of men like Columbus. He claimed to have discovered a new moral continent. His claim is well founded; his political teaching is "wholly new." The only question is whether the new continent is fit for human habitation.[60]

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[51] *What is Political Philosophy?*, p. 27.

[52] Ibid., p. 28.

[53] "Social Science and Humanism," p. 417.

[54] *What is Political Philosophy?*, p. 75.

[55] *What is Political Philosophy?*, p. 75.

[56] *The City and Man*, p. 12.

[57] *What is Political Philosophy?*, p. 40.

[58] Ibid., p. 40.

[59] *On Tyranny*, p. 23.

[60] *What is Political Philosophy?*, p. 40.

Strauss maintains that Machiavelli, in attempting to set out his "new route," deliberately truncated political philosophy by excluding from its scope questions which had previously guided inquiry such as those relating to how human beings ought to live and the character of the best political order. While the classics had little idea that such an order could be actualized except by chance, given the limitations of human nature and usual circumstances, it served as a standard for the old political science which aimed at realizing it insofar as possible. What constituted the tradition-shaking contribution of Machiavelli, which provided the orientation for all modern political philosophy, was his teaching "that one should take one's bearings, not by how men ought to live but by how they actually live." This led to a "lowering of the standards of political life" and an emphasis on overcoming *fortuna* and realizing pragmatic goals. Strauss suggests that this concern with the attainment of "ideals" in the process of human affairs eventually contributed "to the emergence of 'philosophy of history' " and the historicism of the modern age.[61]

Strauss's attack on Machiavelli is passionate. Strauss characterizes him as "a great master of blasphemy," "a devil," and a "teacher of evil" who engaged in "a critique of religion and a critique of morality" in his "diabolical" tract and who rejected virtue as a standard and substituted, at best, the "objectives which are actually pursued by all societies" and, at worst, a set of immoral goals.[62] Strauss observes that it is easy, "after a few centuries of Machiavellianization of Western thought, to give Machiavelli's teaching an air of perfect respectability," and therefore he resolves to demonstrate what a fundamental inversion of the tradition he produced.[63] Modern interpreters of Machiavelli fail to see the immoral character of his teaching and label him a patriot or scientist, but in Strauss's view this is because they are "pupils of Machiavelli" and their work is based on a "dogmatic acceptance of his principles. They do not see the evil character of his thought because they are the heirs of the Machiavellian tradition; because they, or the forgotten teachers of their teachers have been corrupted by Machiavelli."[64] For Strauss, Machiavelli was a conspirator and revolutionary who taught conspiracy and revolution not only against political institutions but against the very tradition itself.

Strauss suggests that already by the time of Machiavelli "the classical tradition had undergone profound changes," but that neither these changes nor the changes effected by Machiavelli and his successors brought increased knowledge. Machiavelli's work involved merely a "contraction of the horizon," presented as if it were a discovery, and was "the first example of a spectacle which has renewed itself in almost every generation since."[65] Strauss argues, however, that Machiavelli initiated an intellectual movement which, in both substance and approach, continued through the modern tradition right up to the *Federalist Papers* and beyond, and his success in dominating the tradition was so profound that Strauss apparently finds no difficulty in speaking of "that Machiavellian, Karl Marx."[66]

Machiavelli's success was outstripped by the subsequent "transformation of his scheme," but this transformation was nevertheless "inspired by his own principle."[67] At times, Strauss seems to elevate Hobbes as the founder of modern political philosophy because of his fundamental break with traditional natural law and other principles of classical political philosophy. In Strauss's rendition of the tradition, Hobbes also receives the title of "originator of modernity." Strauss insists that we must realize that today "our perspective is identical with Hobbes's perspective" and that Hobbes was "the founder of liberalism."[68] But Hobbes's great (or infamous) accomplishment was that he softened the "revolting character" of Machiavelli's teaching in order to make it more palatable and capable of realization.[69] However, even "Hobbes's teaching was still much too bold to be acceptable. It too was in need of mitigation. The mitigation was the work of Locke."[70] Strauss contends that Locke largely propagated Hobbesian principles and that through Locke the doctrines originating with Machiavelli found their way into the thought and action of modern liberal democracy.

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[61] *On Tyranny*, pp. 110–11.

[62] *What is Political Philosophy?*, p. 41; Leo Strauss, *Thoughts on Machiavelli* (Glencoe: Free Press, 1958), pp. 9, 13.

[63] *What is Political Philosophy?*, p. 43.

[64] *Thoughts on Machiavelli*, p. 12.

[65] *What is Political Philosophy?*, p. 43.

[66] Ibid., p. 41.

[67] Ibid., p. 47.

[68] Ibid., p. 172; Leo Strauss, *Spinoza's Critique of Religion* (New York: Schocken Books, 1965), p. 338.

[69] *What is Political Philosophy?*, pp. 47–48.

[70] Ibid., p. 49.

This segment of the tradition, or what Strauss designates as the "first wave of modernity," was complemented by the attack on natural law which evolved out of the French Enlightenment and received its most important expression in the work of Rousseau. "With Rousseau there begins what we may call the second wave of modernity: the wave which bore both German idealistic philosophy and the romanticism of all ranks in all countries."[71] Strauss argues that Rousseau, by his acceptance of the general will as the criterion of political judgment, his denial of any essential nature of man, and his rejection of natural and transcendent standards of justice, fostered a relativism which in effect allows that "cannibalism is as just as its opposite."[72]

The philosophy of history of the nineteenth century, typified by Hegel and Marx, attempted to locate all values in the inner logic and direction of the historical process itself and to demonstrate that right order was an immanent product of that process, independent of purposive human action. But the failure of this vision, the denial of meaning in history, left nothing but the record of the historical process itself and a series of incomparable cultural perspectives. This prepared the way for the emergence of "the third wave of modernity—of the wave that bears us today. This last epoch was inaugurated by Nietzsche," who is "*the* philosopher of relativism," and who in his revolt against a "decayed Hegelianism," laid the foundation for the present situation of radical historicism or the belief, characteristic of existentialism and philosophers such as Heidegger, that human being is "essentially historical."[73] Strauss concludes that, together, these three waves of modernity have culminated in the crisis of our time.

II

Since the scope of this essay does not allow a detailed comparison of Strauss's argument with other secondary literature in the history of political philosophy, my claim regarding a significant generic similarity must remain a hypothesis. But this similarity will be evident to those familiar with the work of Eric Voegelin, Hannah Arendt, and, in varying degrees, numerous other commentators, historians, and textbook authors who have adopted the tradition of political philosophy as a vehicle for their arguments. My principal concern in discussing Strauss is to indicate a characteristic thematic motive of this kind of literature.

Strauss' enterprise is thoroughly instrumental or practical. It is instrumental not only in that he seeks to explain and evaluate the present rather than merely illuminate the past but in that he utilizes the history of political philosophy to mount an attack on what he believes to be the philosophical foundations of contemporary politics. It is perfectly reasonable to make a distinction between what one is doing and the purpose for which one does it, and an understanding of the former is not necessarily dependent on an understanding of the latter. But in Strauss's case they are not separable. His particular reconstruction of the tradition of political philosophy is intelligible only in view of his strategic reasons for undertaking it; that his interpretation of figures such as Locke is designed to impugn the commonly accepted foundation of modern liberalism is too obvious to be ignored. Within the limits of this discussion I cannot demonstrate fully the extent to which Strauss's choice and interpretation of particular works are intended to serve his critique of modernity, but I can indicate the instrumental purpose which informs both his use of the idea of the tradition and his account of the tradition.

Although Strauss puts great emphasis on understanding authors as they understood themselves, this prescription remains, for the most part, at the level of a maxim or the enunciation of an attitude. Apart from his well-known thesis about the relationship between persecution and political theory and the need to look beyond exoteric arguments and apparent inconsistency for arcane meaning, Strauss has little to say about criteria of interpretation. But even this thesis would seem to be more a plausible hypothesis in some instances than a generally applicable principle. He maintains that the fact that all societies rest on prejudice and opinion places limitations on "the philosophers' public speech or writing" and "gives rise to a specific art of writing" which conceals the "beauty of those hidden treasures."[74] Thus "the understanding of this danger and the various forms it has taken, and which it may take, is the foremost task, and indeed the sole task, of the sociology of philosophy."[75] Why Strauss believes that this is the only, and yet imperative, contextual consideration is not revealed. This claim seems principally to place his interpretations based on this premise outside the realm of falsification and debate. This is quite necessary, since his vision of the tradition is an organic one in which the role assigned each figure is essential to the integrity of the entire structure.

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[71] *What is Political Philosophy?*, p. 50.

[72] Ibid., p. 51.

[73] "Relativism," p. 515; *What is Political Philosophy?*, pp. 54–55; *Spinoza's Critique of Religion*, p. 12.

[74] Leo Strauss, *Liberalism, Ancient and Modern* (New York: Basic Books, 1968), p. viii; Leo Strauss, *Persecution and the Art of Writing* (Glencoe: Free Press, 1952), p. 37.

Nowhere does Strauss set forth anything approaching a general theory of textual interpretation or principles of hermeneutical historiography that would give substance to his demand for objectivity. But to approach Strauss's work on the assumption that he is simply engaged in the history of ideas is, despite the great ambiguity regarding the criteria of explanation in this field and notwithstanding the historical form of his argument, to miss the point of what he is doing. Strauss's explication of the tradition of political philosophy is not a research conclusion but a dramaturgical account of the corruption of modernity designed to lend authority to his assertions about the crisis of our time. It is an epic history, complete with epic formulae. Strauss does not demonstrate the existence of the tradition but he assumes it or at least expects his audience to assume it. His description already presupposes the tradition as a datum. He neither discusses what a tradition is nor defends the assumption that the diverse works from Plato to Nietzsche which provide the subject matter of his analysis constitute an inherited pattern of thought with causal implications for contemporary politics and political ideas. He offers neither an account of actual historical connections between the ideas of the figures who comprise this putative tradition nor an explanation of their impact on politics.

Strauss articulates a distinctive tradition myth; but the myth of the tradition, in one form or another, has been a pervasive feature of most of the scholarship in the history of political philosophy. Academic convention selects a basic repertoire of classic works, extending at least from Plato to Marx, arranges them chronologically, infuses them with evolutionary significance, and treats them as a preconstituted tradition which, for better or worse, culminates in modern political theory and practice. Arguments about the history of political philosophy have become arguments about the meaning of this tradition and about the significance of particular works for an understanding of the tradition. Although certain classic texts are always considered essential elements of the tradition, the criteria for inclusion and emphasis usually depend, as does the interpretation of the specific works selected, on a vision of the tradition's total structure.

Despite obvious disparities between the intentions and purposes of particular classic authors, the types of literary composition, and the circumstances of production, these works are approached at a level of abstraction suggesting that they may be understood as belonging to a common genre and concerned with a common set of problems or ideas (representation, freedom, authority, power, justice, obligation, etc.). The presumption is created that significant transitions can be charted by moving from paradigmatic figure to paradigmatic figure. It is suggested that these works may be fruitfully understood, quite literally, as the basic components of a continuing dialogue regarding the great perennial issues of politics and that the differences between these works may be conceived as innovations in the tradition and the similarities construed as continuities.[76]

Historians of political philosophy differ regarding the precise content, meaning, direction, and impact of the tradition. While individuals such as Dunning and Sabine saw a generally progressive development toward scientific knowledge and democracy, despite some aberrations, others, such as Strauss, Arendt, and Voegelin, agree that modernity, and its conception of science and politics, is the product of a tradition that has brought the West to a situation of social and intellectual crisis.[77] While the earlier writers explored a movement toward enlightenment, the latter are engaged in a study of the pathology of the tradition and in a historical subliming operation that seeks to locate the point of derailment in the tradition and to recapture a moment of truth in the past which provides the basis for a critical assessment of modernity and, possibly, an awareness that holds the promise of redemption. The specific intellectual grounds of these synoptic visions of the tradition may differ considerably, but, despite a common rejection of the philosophy of history characteristic of the nineteenth century as the apotheosis of the ideas that have precipitated the modern crisis, they all seem to reflect the structure of the historicist pattern of thought. But whatever the genealogy of these visions may be, the myth of the tradition has contributed to the development of a dominant syndrome in the study of the history of political philosophy.

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[75] *Persecution and the Art of Writing*, p. 21.

[76] Cf. Pocock, *Politics, Language, and Time*, pp. 4–5.

[77] See, for example, Eric Voegelin, *The New Science of Politics* (Chicago: University of Chicago Press, 1952) and *Science, Politics, and Gnosticism* (Chicago: Regnery, 1968); Hannah Arendt, *The Human Condition* (Chicago: University of Chicago Press, 1958), and *Between Past and Future* (New York: Viking, 1968).

The "tradition" is a retrospective analytical construction which produces a rationalized version of the past. It is a virtual tradition calculated to evoke a particular image of our collective public psyche and the political condition of our age, if not the human condition itself. It professes to tell us who we are and how we have arrived at our present situation. As in the case of Strauss's interpretation of Machiavelli, particular works are inevitably approached and understood largely in terms of their assigned role in this tradition and in terms of prior assumptions about the nature of political philosophy. In Strauss's scheme, and in most of the other analyses of this sort, political philosophy, as both action and product, is an ideal typification based on an extrapolation from Greek political thought and general characteristics of the other classic works associated with the tradition. This model is reified and treated as if it were a historically delimited activity in which the individuals selected self-consciously engaged. Thus it becomes possible for "historians" such as Strauss to speak of a beginning, inversion, end or "death," and even a revival of the tradition and to advance what seem to be empirical, rather than merely analytical or categorical, claims about its structure and qualitative features. What emerges is a historical drama, but the import of any substantive version depends initially on the audience's predisposition to accept the tradition as a reality. Strauss consciously plays upon this predisposition, and much of what he has to say involves little more than subtle emendations to received opinion about Socrates and the origin of political philosophy in Greece, Machiavelli as the precursor of modern political science, and the division between ancient and modern thought.

I do not argue that this kind of exercise is illegitimate; however, sometimes those who engage in it may become captives of their own myth while others accept it without realizing its implications or the intentions of its creators. Those educated within the myth of the tradition and who assume that the primary purpose of interpreting a classical text in political philosophy is to illuminate a segment of the tradition, may be unaware of some of the more obvious methodological problems implicit in this approach.

To speak of political philosophy as a tradition begun by Socrates, transformed by Machiavelli, and atrophying in the modern age, or to postulate and characterize a modern political crisis in terms of a catalog of "isms" explained by the evolution of other "isms," is to make reasonable critical discussion impossible. Strauss's description of both the current crisis and the intellectual forces which have produced it lacks contact with concrete events and involves a kind of abstraction which conflicts with his own insistence on confronting ideas on their own terms. He persistently conflates politics and political ideas, and sometimes treats political philosophers as if they were legislators for an age or the tradition and sometimes as if they were representatives of stages in the tradition. Strauss never specifies how individuals such as Machiavelli have effected the great impact on society that he attributes to them, and does not even suggest the evidence substantiating the causal connections between ideas and action to which he continuously alludes.

Similar difficulties are apparent in Strauss's approach to the interpretation of particular works. Although he repeatedly emphasizes the need to understand authors as they understood themselves and rejects those approaches that merely relate their thought to historical situations, he does not systematically confront the manifold problems of textual interpretation involving the relationship between text and context. Strauss approaches authors as having set out to write political philosophy as Strauss defines it and already counts them as participants in the alleged tradition. The substantive "intention" (such as Machiavelli's intention to transform classical political philosophy or Hobbes's intention to ameliorate Machiavelli's teaching) which Strauss attributes to authors is often largely a function of the place Strauss assigns them in the reconstructed tradition rather than something clearly elicited from their work. Although Strauss enjoins interpreters to avoid, and claims that he avoids, "extraneous information," "modern hypotheses," and "conventions" of scholarship and instead move entirely within the "circle of ideas" of authors themselves,[78] it is difficult to see how Strauss carries out this program or, given his preconception about the existence and character of the tradition and his instrumental concerns, could, even in principle, carry it out. The figures he selects to construct the tradition are simply already meaningful within the vision of the tradition he wishes to impart before he sets about interpreting their work.

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[78] *On Tyranny*, p. 25.

Such difficulties characterize much of the literature on the history of political philosophy, but there may be limited relevance in emphasizing these problems. They are relevant only if we assume that the intentions defining the activity of individuals such as Strauss are what would commonly be termed "historical" and "exegetical." What constitutes "historicity" is a complex issue, but this issue may not be the proper context for a critical discussion of much of the secondary literature in this field.

It is indeed strange that Strauss's account of the tradition, with such features as his "wave" metaphor describing the development of modern thought, seems to reflect the very historicism which he so vehemently repudiates. Once pieced together, it is apparent that his story of the decline of the West embodies an extravagant symbolism which plays, in part, upon various eschatological and prophetic motifs. Despite his stress on separating philosophy and history and distinguishing between interpretation and critique, in his approach, philosophical understanding of politics entirely depends on historical analysis, and interpretations of past thinkers are absolutely inseparable from his critique of the present. His condemnation of modernity involves an invocation of its origins and the revelation of a truth that lies buried in the past beneath the dross of the tradition. His assertion that historical investigation is merely a preliminary undertaking to the recovery of the true character of political philosophy and an unadulterated perception of political phenomena and political ends is difficult to accept when most of Strauss's scholarship consists of this type of investigation.

Perhaps he is simply unable to extricate himself from the very intellectual tendencies that he abhors, but it may be more reasonable to suggest that he consciously employs a "historical" argument because he believes he must do so in an age in which only historical arguments carry meaning and authority. It requires very little effort to expose the "methodological" difficulties that may arise from approaching the history of political philosophy from within the framework of the myth of the tradition, and it is more important to indicate how this virtual tradition provides a field of action for the therapeutically inclined historian and serves as a vehicle for his venture in the discovery of error and the recovery of truth.

Since the modern crisis is in large measure the result of historical thinking, Strauss suggests that there is "no more appropriate way of combating this teaching than the study of history" and the employment of a "historical form" of critique.[79] In one place, Strauss argues that "only because public speech demands a mixture of seriousness and playfulness, can a true Platonist present the serious teaching, the philosophical teaching, in a historical and hence, playful garb."[80] My concern is not to conjecture about Strauss's true teaching, but to note the rhetorical function of the myth of the tradition and the instrumental tasks served by this kind of argument. In Strauss's case, the account of the tradition may be employed as a correlate to a philosophical argument, or as a surrogate when discursive argument is inadequate, much as Plato employs mythohistorical tales in his dialogues. From another perspective, what Strauss is about may be likened to what Aristophanes is doing in the *Frogs* when lamenting the passing of tragedy and its role as the educator of society. Just as Aristophanes brings Aeschylus from the underworld to do battle with Euripides, a dramatic confrontation between the old and new teachers of the polis, Strauss descends into history to resurrect the ancients and confront the moderns. In both cases, the moderns, in the existential world, have won the agon, but, in the realm of the drama, the old teachings are vindicated.

Nothing has more obscured the meaning of many of the classic texts in political philosophy, and deflected attention away from the criteria for judging claims about such meaning, than the literature propagating the myth of the tradition and its historical etiology. I do not intend to imply that there are no historical connections between these texts or that there are no traditions of political thought, or even that there is no reasonable sense in which these works could be construed as comprising a tradition. But what has been taken to be *the* tradition is a piece of academic folklore. Even the term "political philosophy" is problematic, since there is no greater interpretive prejudice than approaching these works as if they were philosophical exercises undertaken within the conventions of some particular ongoing activity. There are similarities between these works and similarities between the circumstances and concerns of their authors which justify comparison and generalization and an attempt to isolate them analytically and consider them as a type of literature which can be related to contemporary thought. But it is one thing to engage in a conversation with the past and quite another thing to stage a conversation with the past.

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[79] *On Tyranny*, p. 27.

[80] Leo Strauss, "Farabi's Plato," in *Louis Ginsberg Jubilee Volume* (New York: The American Academy for Jewish Research, 1945), pp. 376–77.

Early Liberal Roots of Feminism: John Locke and the Attack on Patriarchy*

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The seventeenth-century conflict between patriarchal and liberal political thought grew out of a shift in views on the nature of man and society. Sir Robert Filmer insisted that the king ruled absolutely, the divinely ordained father of his people. Sidney, Tyrrell, and Locke grounded political power in an act of consent on the part of free-born individuals.

To what extent did these changing beliefs about human nature also bring about new perspectives on the nature, role and status of women? While this aspect of political theory has usually remained unexplored, it was an important critical weapon in the battle between patriarchal and liberal thought. Patriarchal theorists, relying on the Book of Genesis, could assign women to an appropriately subordinate place in family and society. Consent theories, though, as Filmer noted, depended for consistency on the inclusion of women (as well as children) in the formation of civil society. Early liberal writers including Tyrrell and Sidney were unwilling, then, to abandon patriarchal theory in all its forms and insisted that the consent of the "promiscuous multitude" of women and children was unnecessary. John Locke never specifically dealt with the role of women in the formation of society. Thus, he seemed to have escaped some of the difficulties of finding a place for women consistent with both traditional views of female status and newer views of human nature. Where Locke did speak directly about women, he often departed from traditional views in favor of an individualist approach. Examples of this may be found in his comments on marriage, education and in his discussion of female preaching.

The seventeenth-century conflict between patriarchal and liberal political thought grew out of a shift in views on human nature and the nature of society. This shift eventually led to new perspectives on the nature, role and status of women as well. Though the question of how political theorists handle the subject of women is rarely explored, it may be supposed that the way theorists treat half of humanity should have some consequences for their theories. Yet a theorist's comments on the status of women are usually treated as matters of antiquarian interest only; their significance for a full understanding of a theorist's work is seldom recognized. This was not always the case. In the past, theorists' discussions of women have had important implications for the acceptability of entire theories. The position of women has been used as a critical tool in evaluating theories. The clash between patriarchal and liberal theories serves as a case in point.

Though it is impossible to date the precise origin of the shift in collective consciousness which gave rise to the sexual revolution, it is clear that the revolution itself was (and is) directed against patriarchy. Simply defined, patriarchy is the rule of women by men, and of young men by older men.

Most feminists will agree that economic, social, psychological, political and legal structures are today quite patriarchal in practice. The weight of these structures is particularly oppressive since the theoretical justifications offered in their behalf have been bankrupt for centuries. This bankruptcy became evident, not with Marx or Engels, nor with John Stuart Mill, but with still earlier liberal attacks on the political theory of patriarchy. The vestiges of patriarchalism that survive in contemporary social practice are but remnants of a much more complete form of patriarchalism.

In early seventeenth-century England, patriarchalism was a dominant paradigm, a world view, a weltanschauung.[1] For many English-

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[1] On patriarchalism as a world view, see Gordon J. Schochet, *Patriarchalism and Political Thought* (New York: Basic Books, 1975); also, W. H. Greenleaf, *Order, Empiricism, and Politics* (London: Oxford University Press, 1964), Chs. 1–5; Peter Laslett, "Introduction," *Patriarcha and Other Political Works of Sir Robert Filmer* (Oxford: Basil Blackwell, 1949), p. 26; and John W. Robbins, "The Political Thought of Sir Robert Filmer" (Ph.D. dissertation, The Johns Hopkins University, 1973).

men, it represented the truth of their time and all time. It was a fully articulated theory which expressly accounted for all social relations—king-subject, father-child, master-servant, etc.—in patriarchal terms. Sir Robert Filmer and other patriarchal writers insisted that the king ruled absolutely, the divinely ordained father of his people. No one was born free; everyone was born in subjection to some patriarchal superior. Each individual human being could find his or her proper place by consulting patriarchal theory. Places were not matters of individual choice but were assigned according to a divinely ordained pattern set down at the Creation.

By the end of the seventeenth century, the patriarchal world view had crumbled. It was replaced by a new understanding of human nature and of social and political organization. Whigs such as Sidney, Tyrrell and Locke grounded political power in acts of consent made by free-born individuals. Contract and individual choice supplanted birth and divine designation as crucial factors in social and political analysis. These changes raised problems concerning the status of women in the new order. At first, liberal theorists resisted the suggestion that the old assigned position of women might have to be abandoned. The champions of consent theory saw no need to secure the consent of women. Yet their critics insisted that excluding women violated the very theory of human nature on which liberalism was based. Eventually, liberals would be forced to bring their views on women into line with their theory of human nature. This changing image of women certainly played a part in that shift in consciousness which paved the way for the sexual revolution.

The Statement of Patriarchy: Sir Robert Filmer. While the appearance of full-blown patriarchal political theory was occasioned primarily by the turbulence of seventeenth-century English politics, patriarchal ideas and intimations could be found in political writings long before they received more systematic theoretical expression in the writings of Sir Robert Filmer.[2]

In that era of "divine right kings," the legitimacy of a monarch's claim to absolute rule could be proved if the source of a divine grant of power could be found. Patriarchal political theory satisfied this need. It offered an explanation of the historical origins of the king's political power and of the subject's political obligation. By tracing the king's power back to Adam, the theory provided more than mere historical justification; it provided divine sanction.

The explanation derived its effectiveness from a general awareness of the obvious truth which patriarchalism told.[3] The patriarchal family experience was universal. The family in the seventeenth century was a primary group in every sense of the term. Life was lived on a small scale and the family was at its center. The family patriarch was a universally-acknowledged authority figure with immense power. By linking the authority of the king with the authority of the father, a theorist could immediately clarify the nature of a subject's political obligations. Moreover, monarchical power grounded in patriarchal power took on the legitimacy of that least-challengeable social institution, the family. Patriarchal concepts found in catechism and sermon literature enhanced the king's legitimacy in the eyes of the masses. Finally, the linkage of paternal and monarchical power provided a means for transcending any residual or intermediate loyalties a subject might have. Absolute, patriarchal, monarchical power was vested in the king. It was to the king, not to the local nobility, that loyalty and obedience were rightfully owed.

Patriarchalists insisted that God, nature and history were on their side. For proof, one need only consult the one true account of Creation, namely, the Book of Genesis. Not only was

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[2] Patriarchal strains may be found in the literature of the sixteenth century including John Knox, *First Blast of the Trumpet Against the Monstrous Regiment of Women* (Geneva, 1558). Knox argued that women were incapable of ruling a kingdom. The tract was inspired less by Knox's fear of female rule than his fear of *Catholic* female rule. Counterarguments were introduced by John Aylmer in *An Harborowe for Faithfull and Trewe Subjects against the Late Blown Blast* (Strasborowe, 1559). Patriarchal political theory also influenced James I in *The Trew Law of Free Monarchies* (1598); he noted that "Kings are also compared to Fathers of families: for a King is trewly *Parens Patriae*, the politique father of his people." Richard Field insisted in *Of the Church* (1606) that the political power of Adam as monarch could be derived from his power as father to the whole human race. Patriarchal theorists among Filmer's contemporaries included John Maxwell who wrote *Sacro-Sancta Regum Majestas or the Sacred and Royal Prerogative of Christian Kings* (Oxford, 1644); and James Ussher, *The Power Communicated by God to the Prince, and the Obedience Required of the Subject* (written ca. 1644, first published 1661, 2nd ed., London, 1683); and Robert Sanderson, in his preface to Ussher's work.

[3] Peter Laslett, *The World We Have Lost* (New York: Scribner's, 1965), passim; Greenleaf, pp. 80–94; Peter Zagorin, *A History of Political Thought in the English Revolution* (New York: Humanities Press, 1966), pp. 198–99.

Genesis divinely inspired, it was also the oldest possible historical source and the best guide to man's nature.[4] There, in the Genesis account, was the evidence that God had created Adam in His image—patriarch and monarch He created him.

The gradual unfolding of biblical history showed that the basic institution of patriarchy, the patriarchal family, had always been a fundamental feature of society. Throughout Judeo-Christian society, family life, bolstered by marriage and divorce laws, primogeniture and property rules, continued thoroughly patriarchal down to the seventeenth century.[5]

During the English Civil War, both divine right monarchy and the patriarchal theory which helped support it were severely challenged. Republicans declared that the power of the king was limited. Leveller tracts espoused social contract doctrines. Thomas Hobbes, though himself eager to uphold the power of a strong sovereign, insisted that political power did not originate in a divine grant from God to Adam, father of the race. Instead, Hobbes maintained that political power was the result of an agreement, a covenant among men. In reaction to these novel and dangerous doctrines, Sir Robert Filmer penned the best-known treatises in defense of the patriarchal position.

Filmer was himself something of a model patriarch.[6] He was the eldest son in a family of 17 children, a landholder, a squire of Kent. He married a child heiress in 1610, but did not cohabit with his wife until 1618. Knighted by King James I in 1619, he took over the duties of head of the large Filmer household in 1629. Most of his father's family was dependent on him.

During the Civil War, Sir Robert was seen as a Loyalist. He spent many months in prison and suffered a sizable property loss. After his imprisonment, Filmer published several political tracts including *The Anarchy of a Limited or Mixed Monarchy* (1648), *The Freeholder's Grand Inquest* (1648), and *Observations Upon Aristotle, Touching Forms of Government* (1652). The work for which he is best known, *Patriarcha,* was begun around 1640, but was published posthumously in 1680.[7] Filmer's later political writings often drew on the ideas expressed in this earlier, unpublished manuscript.

To elaborate his patriarchal theory of politics, Filmer turned to both classical and constitutional sources. But Filmer's most important, most authoritative source was always scripture. Aristotle could be used as a commentary; but whenever disagreement arose between Aristotle and Filmer, or Aristotle and scripture, Sir Robert would summon the force of scripture behind his own arguments, reminding that "heathen authors" were ignorant of the true facts of creation and the beginnings of government (F.W. 187).

The scriptural arguments for monarchy illustrate the most literally patriarchal aspects of Filmer's thought. In brief, his account of the biblical origins and justifications of patriarchy was as follows:

> God created only Adam, and of a piece of him made the woman; and if by generation from them two as parts of them, all mankind be propagated: if also God gave to Adam not only the dominion over the woman and the children that should issue from them, but also over the whole earth to subdue it, and over all the creatures on it, so that as long as Adam lived no man could claim or enjoy anything but by donation, assignation, or permission from him (F.W. 241).

Again and again throughout his works Filmer recalled the divine grant of paternal, monarchical power to Adam. Filmer drew upon the Book of Genesis, specifically Genesis 1:28, when he claimed that "the first government in the world was monarchical in the father of all flesh. Adam being commanded to multiply, and people the earth and subdue it, and having dominion given him over all creatures, was thereby the monarch of the whole world" (F.W. 187).

As critics from Filmer's own century were only too happy to observe, Sir Robert had erred in his biblical analysis. Filmer had assigned all power to Adam, but God had given dominion to Adam and Eve. The divine grant of power in Genesis 1:28 was made to "them," ostensibly the male and female whose creation had been announced in the preceding verse. Sir

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[4] On the use of scripture in historical argument see J. G. A. Pocock, *The Ancient Constitution and the Feudal Law* (Cambridge: Cambridge University Press, 1957), pp. 188–89.

[5] See especially Greenleaf, p. 89; also Julia O'Faolain and Laura Martines, eds. *Not in God's Image* (New York: Harper Torchbooks, 1973), pp. 179–207; and Schochet, p. 16.

[6] Laslett, "Introduction," and Laslett, "Sir Robert Filmer: The Man Versus the Whig Myth," *William and Mary Quarterly,* 3rd ser., 5 (1948), 523–46.

[7] Sir Robert Filmer, *Patriarcha and Other Political Writings of Sir Robert Filmer,* ed. Peter Laslett (Oxford: Basil Blackwell, 1949). This volume will be cited in the text as "F.W."

Robert had to tamper with the text because the original grant of power detailed in Genesis 1:28 was not, as he maintained, an exclusive grant of private monarchical dominion given to Adam, the patriarch. On the contrary, the blessing was given to both the male and the female.

If evidence for the patriarchal theory could not be found in God's blessing, perhaps it could be found in His curse. Conceivably, Filmer might have agreed that the original prelapsarian grant had been made to Adam and Eve jointly, and might still have salvaged his patriarchal theory. He could have maintained that the lines of patriarchal authority were established only after the Fall. Genesis 3:16 could have been offered as proof: "Thy desire shall be to thy husband, he shall rule over thee."

Indeed, in the *Anarchy*, Filmer did refer to these lines as proof that "God ordained Adam to rule over his wife . . . and as hers so all theirs that should come of her" (F.W. 283). Nevertheless, it is clear that Sir Robert preferred the Genesis 1:28 passage. By using that text, he could show that patriarchal order was in accord with man's original nature, not simply with his fallen nature. Filmer hoped to show that the human hierarchy was established in the *very* beginning. Every moment that passed risked the introduction of a custom which might lend credence to the anti-patriarchal case.[8] Each passing second made monarchical power appear less natural, and shared dominion more legitimate. Consequently, Filmer preferred to insist that Adam was monarch of the world from the very first moment of creation:

> By the appointment of God, as soon as Adam was created he was monarch of the world, though he had no subjects; for though there could not be actual government until there were subjects yet by the right of nature it was due to Adam to be governor of his posterity: though not in act, yet at least in habit. Adam was a King from his creation: and in the state of innocency he had been governor of his children; for the integrity or excellency of the subjects doth not take away the order or eminency of the governor. Eve was subject to Adam before he sinned; the angels who are of a pure nature, are subject to God (F.W. 289).

Filmer's analogy here is striking. The qualitative difference between man and woman, between king and subject, is virtually infinite; indeed, it parallels the qualitative difference which exists between God and the angels.

Genesis was not the only biblical source of patriarchal theory. The Decalogue, too, served to support patriarchal political authority, according to Filmer: "The power of the government is settled and fixed by the commandment of 'honour thy Father'; if there were a higher power than the fatherly, then this command could not stand and be observed" (F.W. 188). Filmer's omission is obvious. In service of political patriarchalism, the last half of the fifth commandment was dropped. All honor due to mother was forgotten.

Filmer and the Contract Theorists. Filmer's habit of selective quotation was not overlooked by his critics. In the 1680's Whigs severely attacked *Patriarcha* by dredging up one biblical reference after another to prove Sir Robert had flagrantly abused scriptural texts to support his theory. Lengthy criticisms were written by Edward Gee, a contemporary of Filmer's, and by later Whig theorists including Algernon Sidney, James Tyrrell, and John Locke.[9] Biblical criticisms constituted a major part of their attack on patriarchal theory. Indeed, Filmer's critics actually did far more biblical exegesis than he did.[10] Later commentators saw Filmer's opponents' efforts as little more than intellectual overkill. The commentators failed to realize that Filmer's thought was part of a pervasive pattern which had had a tight grip on the minds of many Englishmen.

Filmer's critics had to attack him by striking at the theological-scriptural base of the theory since scripture was by far its most important source. Filmer used scripture as the ultimate weapon, guaranteed to clinch any argument. In the eyes of his fellow Englishmen who shared his world view, the only way Sir Robert could be refuted was by destroying his scriptural base.[11]

In the course of the seventeenth century, standards of evidence and styles of argument changed dramatically. Forms of argument which had been perfectly acceptable, indeed even indispensable, in earlier political discourse were rejected in favor of newer "rational" arguments. Although John Locke would champion the new mode of thought, the old form still had a hold on him. Locke took Filmer's biblical arguments seriously, as challenges to be met and overcome. Locke's attack on Filmer, though incomplete, gives the impression that once the biblical criticism was finished, he believed Filmer stood refuted and the attack on contract theory rebutted. This was not necessarily true.[12]

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[8] Pocock, pp. 189–90.

[9] See, for example, Edward Gee, *The Divine Right and Original of the Civil Magistrate from God* (London, 1658); [James Tyrrell], *Patriarcha Non Monarcha* (London: Richard Janeway, 1681); and Algernon Sidney, *Discourses Concerning Government* (London, 1698).

[10] Greenleaf, p. 89.

[11] Arguments had to be structured to persuade the widest possible audience. For an exploration of this general problem, see Mark Gavre, "Hobbes and His Audience," *American Political Science Review*, 68 (December 1974), 1542–56.

Filmer staunchly insisted that man was not by nature free. Rather, man was born to subjection:

> Every man that is born is so far from being free-born that by his very birth he becomes a subject to him that begets him: under which subjection he is always to live, unless by immediate appointment from God, or by the grant or death of his Father, he becomes possessed of that power to which he was subject (F.W. 232).

By looking to the Garden of Eden, Filmer thought he could demonstrate the truth about natural man and his natural forms of association, but his contention that patriarchal monarchy was the natural and only legitimate form of government did not receive its force solely from the scriptural account. Sir Robert also relied on constitutional and classical sources to complement his biblical evidence. More importantly, however, the claims he made were strengthened by their apparent empirical relevance. The paternal power of the father and of the king was evident to all who would but look about them. The experience of patriarchalism in family life was a constant, natural feature of daily life. So too, paternal power in a kingdom would remain constant: "There is and always shall be continued to the end of the world, a natural right of a supreme Father over every multitude" (F.W. 62).

So persuasive was the use of patriarchy as a legitimating concept that Filmer maintained that even God exercised power by right of fatherhood. Filmer's argument takes on a circular character at this point. Nevertheless, it is clear that there was absolutely no room in patriarchal theory for free-born individuals. Government could not begin with an act of consent made by free and equal individuals in a state of nature. Filmer insisted that such government could be based on no more than myth. Furthermore, he insisted that contract theories which advanced such a myth would be replete with contradictions and logical fallacies.

Filmer offered a theory which was truly comprehensive and coherent, one which provided a place for every individual in society. His opponents, on the other hand, were far less able to provide a satisfactory accommodation for all the individuals and groups which made up seventeenth-century English society.

At least part of the difficulty which his adversaries encountered stemmed from their inability to dismiss patriarchalism completely in all its manifestations. They wished to destroy the patriarchal base of monarchy, and sever the connection between patriarchalism and divine-right politics, yet they were unable to reject less comprehensive forms of patriarchalism as basic organizing principles of government and society. They developed a new theory of human nature, but did not foresee or develop the implications of that theory. This point can be illustrated specifically by examining the position of women in their theories.

Whig theorists such as Tyrrell and Sidney certainly believed that whole classes of people were plainly unfit to exercise political power and plainly incapable of giving their own consent to government. Sidney spoke harshly of forms of government which granted power without regard to age, sex, infirmity or vice.[13] Tyrrell wrote, "There never was any Government where all the Promiscuous Rabble of Women and Children had Votes, as being not capable of it."[14]

Edward Gee would have founded his "democracy" on fatherly authority. Those who lacked political significance (i.e., women, children, servants) would be "involved" in their patriarchal superiors.[15] James Tyrrell indicated that he found a similar arrangement acceptable at the foundation of a commonwealth.[16] In accepting this sort of "democracy" these theorists were not very far from Filmer. Sir Robert permitted the patriarchal heads of families to gather together to decide who among them would become supreme father when the king died without an heir (F.W. 288). Filmer, Gee, Tyrrell and Sidney all agreed on one thing: women and children need have no part in these political decisions.

Despite their criticisms of patriarchalism and their arguments based on consent, neither Tyrrell nor his friend, John Locke, was willing to allow participation to all comers. Tyrrell wished to limit participation to male property owners. Locke, as MacPherson argues, would have limited participation to the demonstrably rational (read "acquisitive") classes.[17] But these limitations were swept away by historical actualities over the next two centuries. Rights to political participation were gradually extended to all men and subsequently to all women. Indeed, Filmer, rather than Locke or Tyrrell proved the better predictor of the historical course plotted by the liberal logic when he wrote of government by the people:

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[12] Laslett concluded that "neither Locke nor Sidney nor any of a host of others who attacked *Patriarcha* ever attempted to meet the force of [Filmer's] criticisms [about political obligation], and that none of them ever realized what he meant by his naturalism." Introduction, p. 21.

[13] Sidney, pp. 2–4, 34–35.

[14] Tyrrell, p. 83.

[15] Schochet, p. 233.

[16] Tyrrell, p. 74.

> If but one man be excluded, the same reason that excludes one man, may exclude many hundreds, and many thousands, yea, and the major part itself; if it be admitted, that the people are or ever were free by nature, and not to be governed, but by their own consent, it is most unjust to exclude any one man from his right in government (F.W. 211).

No one could be excluded from political participation if contract theorists were to remain true to their principles. Filmer understood that in speaking of "the people" and their natural liberty, one had to talk about all mankind.

Though contract theorists came to consider their theories as logical or moral rather than as historical, Filmer used the historical problems of the social contract in an attempt to undermine the logical and moral status of the theory. Filmer insisted that the state of nature and the social contract became logically and historically unacceptable doctrines if "the people" were to be equated with "all mankind." Furthermore, he believed that contract theorists themselves would recoil when faced with the full implications of their theory.

Filmer demanded to know the details of the great meeting where the contract was approved. When did the meeting occur? Who decided the time and place? More importantly, he wanted to know who was invited. Filmer saw these as serious problems for consent theorists since:

> Mankind is like the sea, ever ebbing or flowing every minute one is born another dies; those that are the people this minute are not the people the next minute, in every instant and point of time there is a variation: no one time can be indifferent for all of mankind to assemble; it cannot but be mischevious always at least to all infants and others under the age of discretion; not to speak of women, especially virgins, who by birth have as much natural freedom as any other and therefore ought not to lose their liberty without their consent (F.W. 287).

Filmer's attack was no longer simply historical; it was now logical and moral as well. It was clear to him that if the "natural freedom" of mankind was to be taken seriously, obviously the natural freedom of women and children would have to be considered. If women and children were free, they would have to be included in any sort of compact. "Tacit consent" was an impossibility, and was rejected by Filmer as "unreasonable" and "unnatural" (F.W. 225). Simply to "conclude" the votes of children, for example, in the votes of parents would not be adequate:

> This remedy may cure some part of the mischief, but it destroys the whole cause, and at last stumbles upon the true original of government. For if it be allowed that the acts of the parents bind the children, then farewell the doctrine of the natural freedom of mankind; where subjection of children to parents is natural there can be no natural freedom (F.W. 287).

Filmer would probably have agreed that the same line of reasoning could be used to analyze the relationship of women to the social contract.

Filmer's technique in this instance was one of his favorites—reductio ad absurdum. Natural freedom and political participation of women and children were obviously absurd to all living in that patriarchal world. Thus, Filmer's case against natural freedom and the social contract stood proved. His aim was to show the absurdity of the concept "consent of all the people." He insisted that "all the people" must be taken at face value. It must include groups of people obviously unfit for such decision making, that is, children, servants and *women*. Each of these groups had been accorded a place within the social and political theory of patriarchy. Each group's place was in accord with a traditional evaluation of its status.

Those who asserted the natural freedom of all mankind upset the applecart. If men were born free and equal, status could not be ascribed at birth, but would have to be achieved in life. If Filmer's opponents were to be consistent, new political roles would have to be opened up for those previously judged politically incompetent. This consequence was never fully clear to Filmer's critics. Though

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[17] C. B. MacPherson, *The Political Theory of Possessive Individualism* (London: Oxford University Press, 1962), Ch. 5; and MacPherson, "The Social Bearing of Locke's Political Theory," *Western Political Quarterly*, 7 (March 1954), 1–22.

Tyrrell and Sidney criticized Filmer's patriarchalism, they were by no means ready or willing to break with all the trappings of patriarchy. Consequently, they faced additional difficulties when they tried to account for the political obligation of the politically incompetent.

Tyrrell attempted to base the political obligation of women and children on the debt owed for their "breeding up and preservation." At first glance, this formula may seem to resemble Locke's doctrine of "tacit consent." A closer look will reveal that Tyrrell's version is far more extensive than Locke's. Tyrrell demanded much more from women and children than simply the passive obedience required of sojourners who were not parties to the compact:

> They may well be lookt upon as under an higher Obligation in Conscience and in Gratitude to his Government, than Strangers of another Country, who onely staying here for a time to pursue their own Occasions and having no Right to the same privileges and advantages of the Commonwealth do onely owe a passive obedience to its laws.[18]

The obligation of disenfranchised groups stemmed from their nurture, from the debt of gratitude owed to the government for their upbringing and education. They had no actual voice. They themselves were never expected to give free consent to their government. Yet still they were held to be obliged—out of gratitude.

This sort of obligation theory is not far removed from Filmer's. The natural duties of Filmer's king were "summed up in a universal fatherly care of his people" (F.W. 63). The King preserved, fed, clothed, instructed, and defended the whole commonwealth. Government by contract would do the same things for those who were not part of the contract. In return for these services alone, political nonparticipants owed "a higher Obligation in conscience and gratitude." No participation, no express consent was necessary to put an end to their natural freedom.

A third problem was created for both Filmer and his critics when the questions of participation and monarchical succession were considered together. Filmer did not use patriarchal theory to challenge women's claims to the throne. His critics, especially Sidney, seized upon his silence, protesting that Filmer would allow even women and children to rule as patriarchs. Patriarchal theory enthroned "the next in Blood, without any regard to Age, Sex or other Qualities of the Mind or Body."[19]

Whig theorists did not render Filmer's arguments less damaging to their cause, but they did turn them back on patriarchal theory. To Filmer, contract theory was absurd because it entailed the participation of politically unfit groups in the formation of government and society. To Whigs, the patriarchal position was outrageous because it risked giving a single, similarly incompetent individual absolute unchecked dominion.

To summarize, both Whig and patriarchal theorists used the position of women as a critical tool in evaluating competing theories. Both Whig and patriarchal theorists had to find places for women in their theories. Each criticized the other for the role and status eventually assigned to women.

In patriarchal theory, women held a distinctly subordinate position. Their inferior place in family, state and society was justified on the basis of scriptural exegesis. But scripture was frequently twisted in service of patriarchalism.

The errors and excesses of Filmer's theory were pointed out gleefully by his critics, yet many of these critics rejected only political patriarchalism, and that only in a limited way. Whigs stripped Genesis of its political import. They freed both men and women from subjection to a supreme earthly father. The social implications of Genesis were not completely rejected, however. The story of the Creation and Fall was still used to show the general inferiority of women to men. In practical terms, Whigs initiated no real attack on Filmer's discussion of women. They attacked the theological-historical justifications for political patriarchy, but basically supported the institutional arrangements promoted by patriarchy where women were concerned.

The issue of women's political participation created problems for both Whigs and patriarchs. Patriarchalism was a conservative theory aimed at legitimation of the reigning monarch. Patriarchal legitimacy was founded in an argument which was inherently antifemale. Yet English law permitted women to reign as sole monarchs. Patriarchal theorists found no really satisfactory escape from this dilemma.

Whigs upheld the natural freedom of all mankind yet maintained that women's consent to government could be "concluded" in that of their husbands or fathers. Whigs rejected the biblical basis for political subordination of women, but accepted empirically-based arguments which showed women naturally unfit for political life.

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[18] Tyrrell, p. 78.

[19] Sidney, p. 4.

In effect, Whigs substituted a community of many patriarchs for Filmer's supreme patriarch. Filmer, the patriarch, realized immediately that this simple substitution alone was much less than was required by the doctrine of natural freedom of all mankind. Slowly, over the next two centuries, even liberal thinkers would be drawn to the same conclusion.

Locke's Attack on Patriarchy. While other Whig writers simply declared that their theories necessitated no new roles for women, John Locke treated the problem somewhat differently. He was among the first to sense the inherent contradiction in a "liberalism" based on the natural freedom of mankind, which accorded women no greater freedom than allowed by patriarchalism. New places had to be opened to women. This is not to claim that John Locke planned or even foresaw the feminist movement. It does seem true, however, that Locke took his individualist principles very seriously, even when they entailed an admission that women, too, might have to be considered "individuals."

Clearly Locke was not interested in creating a world in which all were equal; in his view, there would always be differences among individuals. The key question here concerns the extent to which a Lockean society would discriminate on the basis of sex. Would the fact that some are more equal than others be determined by traditionally-assigned sex roles?

In the first of his *Two Treatises of Government,* Locke showed little interest in the constitutional or classical arguments offered by Filmer. Doubtless he believed that these arguments were simply not at the heart of Filmer's theory. Instead, Locke charged that the scripture-based arguments were unproved, not because he doubted the truth of the Bible, but because he realized Filmer had distorted that truth. Locke's attack stemmed from no impious disregard for Filmer's evidence, but from a different method of construing that evidence. As Laslett suggests, Locke had broken the bounds of Filmer's world of biblical politics by introducing rationalist arguments.[20]

Since Filmer's patriarchal theory included a particular view of the status of women, based on biblical arguments, Locke's refutation also had to deal with that view. Concerning the benediction of Genesis 1:28, Locke noted that it was bestowed on "more than one, for it was spoken in the Plural Number, God blessed *them* and said unto *them,* Have Dominion. God says unto *Adam* and *Eve,* Have Dominion" (T.T., I, 29). This argument introduced the possibility that Adam's dominion was not exclusive but was shared with Eve. Further, Eve's subjection to Adam need not have prevented her from exercising dominion over the things of the Earth. Eve, too, might have property rights.

In the fifth chapter of the *First Treatise,* Locke argued against "Adam's title to Sovereignty by the Subjection of Eve." There, Locke had much more to say about the patriarchal conception of women. He took issue with Filmer's use of Genesis 3:16 ("And thy desire shall be to thy Husband and he shall rule over thee"). Those words, Locke objected, were a "punishment laid upon Eve." Furthermore, these words were not even spoken to Adam. The moment after the great transgression, Locke noted, "was not a time when Adam could expect any Favours, any grant of Priviledges from his offended Maker." At most, the curse would "concern the Female Sex only," through Eve, its representative (T.T., I, 45–47).

Here, Locke argued that Genesis 3:16 offered no evidence of a general grant of power to Adam over all mankind. By limiting the curse to Eve and to women, Locke effectively removed males from the sway of the patriarchal monarch. But he went even further, and suggested that the arguments for the subjection of women based on the Genesis 3:16 passage could be faulty.

First, the subjection of women carried no political import. The curse imposed "no more [than] that Subjection they [women] should ordinarily be in to their Husbands." But even this limit on women's freedom was not immutable and could be overcome:

> There is here no more Law to oblige a Woman to such a Subjection, if the Circumstances either of her Condition or Contract with her Husband should exempt her from it, then there is, that she should bring forth her Children in Sorrow and Pain, if there could be found a remedy for it, which is also part of the same Curse upon her (T.T., I, 47).

Nevertheless, Locke largely accepted the empirical fact of women's inferiority and saw it grounded in nature as ordered by God. He attempted to avoid the conclusion that Adam became Eve's superior or that husbands became their wives' superiors, yet his effort is fairly weak:

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[20] Peter Laslett, "Introduction," to *Two Treatises of Government,* written by John Locke, ed. Laslett (Cambridge: Cambridge University Press, 1960), p. 69. References to Locke's *Treatises* will be made to "T.T." with the treatise and section numbers indicated.

> God, in this Text, gives not, that I see, any Authority to Adam over Eve, or to Men over their Wives, but only foretells what should be the Woman's Lot, how by his Providence he would order it so, that she should be subject to her husband as we see that generally the Laws of Mankind and customs of Nations have ordered it so; and there is, I grant, a Foundation in Nature for it (T.T., I, 47).

Locke was principally interested in refuting the idea of a divine grant of authority to Adam. He lived in a world in which the subjection of women was an empirical fact and he was willing to yield to the contemporary view that this fact had some foundation in nature. His tone was hesitant, though. Locke seemed to wish that God had not been responsible for women's inferior status. He tried to cast God in the role of prophet rather than creator. God merely "foretold" what women's lot would be. Locke found it difficult to keep God in the role of innocent bystander, however. Where Locke admitted the use of divine power, he tried to remain tentative: God, in his Providence, "would order" social relations so that wives would be subject to their husbands. But God did not give men any kind of rightful authority over women. Locke implied that God merely suggested one empirical relationship which was subsequently adopted by mankind and reinforced by the laws and customs of nations. That these laws and customs were largely established by males did not, in Locke's opinion, damage the case. It did not seem to bother him that such laws and customs offered proof of the authority which men exercised over women. Locke simply wished to deny that male authority was exercised by virtue of some divine grant. At this point, he had no need to reject the customary exercise of such authority. It was enough to show only that it was human and not divine in origin.

Peter Laslett notes that "Locke's attitude towards the curse on women in childbearing is typical of his progressive, humanitarian rationalism."[21] But Locke's views on women were also evidence of his individualism. Though Locke believed there was a "foundation in nature" for the limitations on women, he remained faithful to the individualist principles which underlay his theory. In his view, women were free to overcome their natural limitations; each woman was permitted to strike a better deal for herself whenever possible.

In conjunction with his attack on Filmer's use of Genesis 3:16, Locke touched another of patriarchy's soft spots. He sensed the weakness of Filmer's insistence on the inferiority of women in a nation where women had worn the crown. Locke made no sustained analysis of this point, but remarked, instead, "[will anyone say] that either of our Queens *Mary* or *Elizabeth* had they Married any of their Subjects, had been by this Text put into a Political Subjection to him? or that he thereby should have had Monarchical Rule over her?" (T.T., I, 47)

Locke also accused Sir Robert of performing procrustean mutilations of "words and senses of Authors" (T.T., I, 60). This tendency was most evident in Filmer's abbreviation of the fifth commandment. Filmer had cited the command in several places throughout his works, but always in the same terms, "Honour thy Father." Locke noted this fact and then complained that "and Mother, as Apocriphal Words, are always left out." Filmer had overlooked the "constant Tenor of the Scripture," Locke maintained. To bolster his position, Locke produced well over a dozen scriptural citations showing the child's duty to father and mother. A mother's title to honor from her children was independent of the will of her husband. This independent right, he argued, was totally inconsistent with the existence of absolute monarchical power vested in the father (T.T., I, 63). Ultimately, Locke denied that the fifth commandment had any political implications at all (T.T., I, 65).

In this analysis, Locke broke with one of patriarchy's strongest traditions. Political obligation had been justified through the fifth commandment. In seventeenth-century sermon literature and catechism texts, the subject's duty of obedience was firmly rooted in this command. Locke refuted these arguments, not by criticizing the use of scriptural evidence, but by analyzing the interpretations supposedly based on that source.

This completed the destructive part of Locke's case. His attack rent the fabric of Filmer's theory. Since patriarchalism represented a complete, integrated theory of society, an adequate successor-theory would have to replace all its shattered parts. If all social relations could no longer be understood through the patriarchal paradigm, how could they be understood? Locke's answer came in the *Second Treatise*. There he made his positive contribution to the understanding of social relations.

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[21] Laslett, ed., *Two Treatises*, p. 210 n.

Social Relations in the Second Treatise. For Filmer and his sympathizers there was only one type of power: paternal power. This power was, by its nature, absolute. Filmer's simplistic, uncluttered view of power fit in perfectly with his analysis of social relations. Filmer admitted only one kind of social relationship: the paternal relationship. Each member of society was defined by his or her relation to the patriarchs of the family and of the nation.

Locke, however, maintained that there were many kinds of power and many types of social relations. He analyzed several nonpolitical relationships including those of master-servant, master-slave, parent-child, and husband-wife.[22] Each of these forms of association was carefully distinguished from the political relationship of ruler-subject. Two of the nonpolitical relationships, namely the parental and the conjugal, reveal a great deal about the status of women in Lockean theory.

From the very outset of the discussion of the parent-child relation, Locke rejected the terminology of patriarchy, claiming that "[paternal power] seems so to place the Power of Parents over their Children wholly in the Father, as if the Mother had no share in it, whereas if we consult Reason or Revelation, we shall find she hath an equal Title.... For whatever obligation Nature and the right of Generation lays on Children, it must certainly bind them equal to both the concurrent Causes of it" (T.T., II, 52).

The basic argument at the root of his terminological objection was one familiar from the *First Treatise*. Patriarchal theory could not stand if power were shared by husband and wife. As Locke argued in the *Second Treatise*, "it will but very ill serve the turn of those Men who contend so much for the Absolute Power and Authority of the *Fatherhood*, as they call it, that the *Mother* should have any share in it" (T.T., II, 53). Nevertheless, Locke was not consistent in his own use of the term he introduced. He reverted to the use of "paternal" to describe the relationship he defined as "parental." Yet it is clear from this discussion as well as from the analysis of the fifth commandment that Locke was willing to elevate women's status if he could overthrow the patriarchal monarch.

Locke's examination of the conjugal relationship demanded a more extensive analysis of the roles and status of women in society. He described conjugal society as follows:

> *Conjugal Society* is made by a voluntary Compact between Man and Woman: tho' it consist chiefly in such a Communion and Right in one anothers Bodies, as is necessary to its chief End, Procreation; yet it draws with it mutual Support and Assistance, and a Communion of Interest too, as necessary not only to unite their Care, and Affection, but also necessary to their common Off-spring, who have a Right to be nourished and maintained by them, till they are able to provide for themselves (T.T., II, 78).

Conjugal society existed among human beings as a persistent social relationship because of the long term of dependency of the offspring and further because of the dependency of the woman who "is capable of conceiving, and *de facto* is commonly with Child again, and Brings forth too a new Birth long before the former is out of a dependency" (T.T., II, 80). Thus the father is obliged to care for his children and is also "under an Obligation to continue in Conjugal Society with the same Woman longer than other creatures" (T.T., II, 80).

Though the conjugal relationship began for the sake of procreation, it continued for the sake of property. After praising God's wisdom for combining in man an acquisitive nature and a slow maturing process, Locke noted that a departure from monogamy would complicate the simple natural economics of the conjugal system (T.T., II, 80). Though conjugal society among human beings would be more persistent than among other species, this did not mean that marriage would be indissoluble. Indeed, Locke wondered "why this *Compact*, where Procreation and Education are secured, and Inheritance taken care for, may not be made determinable, either by consent, or at a certain time, or upon certain Conditions, as well as any other voluntary Compacts, there being no necessity in the nature of the thing, nor to the ends of it, that it shall always be for life" (T.T., II, 81). Locke's tentative acceptance of divorce brought him criticism over 100 years later. Thomas Elrington commented that "to make the conjugal union determinable by consent, is to introduce a promiscuous concubinage." Laslett notes that Locke was prepared to go even further and suggested the possibilities of left-hand marriage.[23] In Locke's view, the actual terms of the conjugal contract were not fixed and immutable:

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[22] See especially R. W. K. Hinton, "Husbands, Fathers, and Conquerors," *Political Studies*, 16 (February 1968), 55–67; Geraint Parry, "Individuality, Politics and the Critique of Paternalism in John Locke," *Political Studies*, 12 (June 1964), 163–77; and MacPherson, *Possessive Individualism*.

[23] Laslett, ed., *Two Treatises*, p. 364, n.

> Community of Goods and the Power over them, mutual Assistance and Maintenance, and other things belonging to *Conjugal Society*, might be varied and regulated by that Contract, which unites Man and Wife in that Society as far as may consist with Procreation and the bringing up of Children (T.T., II, 83).

Nevertheless, Locke described what he took to be the normal distribution of power in marital relationships:

> The Husband and Wife, though they have but one common Concern, yet having different understandings will unavoidably sometimes have different wills, too; it therefore being necessary, that the last Determination, *ie.* the Rule, should be placed somewhere, it naturally falls to the Man's share, as the abler and the stronger (T.T., II, 82).

Clearly all forms of patriarchalism did not die with Filmer and his fellows. Here, the subjection of women is not based on Genesis, but on natural qualifications. Nature had shown man to be the "abler and stronger." Even James Tyrrell, while denying any need to obtain women's consent in the formation of civil society, thought it possible that, in some cases, women might actually be more fit to act as household heads and final decision makers. Unlike Tyrrell, Locke did not equivocate on this point. Rule must be placed somewhere, so he placed it in the husband. Locke's patriarchy was limited, though. The husband's power of decision extended only to those interests and properties held in common by husband and wife. Locke spelled out the limits on the husband's power:

> [His power] leaves the Wife in the full and free possession of what by Contract is her Peculiar Right, and gives the Husband no more power over her Life, than she has over his. The *Power of the Husband* being so far from that of an absolute monarch that the *Wife* has, in many cases, a Liberty to *separate* from him; where natural Right or their Contract allows it, whether that Contract be made by themselves in the state of Nature or by the Customs or Laws of the Country they live in; and the Children upon such Separation fall to the Father or Mother's lot, as such contract does determine (T.T., II, 82).

In addition, Locke distinguished between the property rights of husband and wife. All property in conjugal society was not automatically the husband's. A wife could have property rights not subject to her husband's control. Locke indicated this in a passage on conquest: "For as to the Wife's share, whether her own Labour or Compact gave her a Title to it, 'tis plain, her Husband could not forgeit what was hers" (T.T., II, 183).

There were several similarities between the conjugal and the political relationship. Both were grounded in consent. Both existed for the preservation of property. Yet conjugal society was not a political society because it conferred no power over the life and death of its members. In addition, political society could intervene in the affairs of conjugal society. Men and women in the state of nature were free to determine the terms of the conjugal contract. But in civil society these terms could be limited or dictated by the "Customs or Laws of the Country."

The extent to which the participants in the parental and conjugal relationships could also participate in political relationships remains to be considered. We may gain some insight into the matter by following Locke's route, that is, by tracing the origins of political power from the state of nature.

To Locke, the state of nature was a "state of perfect Freedom" for individuals "to order Actions and dispose of their Possessions, and Persons, as they think fit." Furthermore, Locke also described the state of nature as:

> A *State* also of Equality, wherein all the Power and Jurisdiction is reciprocal, no one having more than another: there being nothing more evident, than that Creatures of the same species and rank promiscuously born to all the same advantages of Nature and the use of the same faculties should also be equal one amongst another without Subordination or Subjection, unless the Lord and Master of them all should by any manifest Declaration of his Will set one above another (T.T., II, 4).

Because of certain inconveniences (the lack of an authoritative executive power, the potential for injustice where men judged their own cases, etc.), men quit the state of nature to form civil society through an act of consent. It was in criticizing the formation of society by consent that Filmer's theory was most effective. Indeed, Locke found it difficult to show how free and equal individuals actually formed civil society. Ultimately he was forced to admit that the first political societies in history were probably patriarchal monarchies. He described the historic origins as follows:

> As it often happens, where there is much Land and few People, the Government commonly began in the Father. For the Father having by the Law of Nature, the same Power with every Man else to punish his transgressing Children even when they were Men, and out of their Pupilage; and they were very likely to submit to his punishment, and all joyn with him against the Offender in their turns, giving him thereby

> power to Execute his Sentence against any transgression . . . [the] Custom of obeying him, in their Childhood made it easier to submit to him rather than to any other (T.T., II, 105).

In this passage, Locke lumped paternal power and natural power together, allowed for the slightest nod of consent, and–presto–civil society emerged. Even in the state of nature, it appeared that paternal (parental?) power could be effective. Children growing up in the state of nature were under the same obligations to their parents as children reared in civil society. What of natural freedom and equality? Locke confessed:

> *Children* are not born in this full state of *Equality,* though they are born to it. Their parents have a sort of Rule and Jurisdiction over them when they come into the World, and for some time after, but 'tis but a temporary one. The Bonds of this Subjection are like Swadling Cloths they are wrapt up in and supported by in the weakness of their Infancy. Age and Reason as they grow up, loosen them till at length they drop quite off, and leave a Man at his own free Disposal (T.T., II, 55).

Of course, once children reached maturity in the state of nature they no longer owed obedience to their parents, but were simply required to honor them out of simple gratitude. At this stage, however, Locke introduced another sort of power to support the father's claim to his child's obedience–namely that power which accrued to every man in the state of nature, the power to punish the transgressions of others against him. But the father's power was reinforced by the fact that his children would have a long-standing habit of obedience to him. In the state of nature, the father's commands to his mature children received added weight and legitimacy because he *was* their father. His children would recognize this legitimacy and would join their power to his to make him lawmaker. At this point, it seems, the father's former paternal power and his existing natural power were transformed by consent into political power.

Locke's account of the origins of political power raised problems. First of all, it seemed that the consent of free and equal individuals came about more as a result of habit than of mature, rational deliberation. Secondly, Locke's reconstruction fell prey to one of the difficulties he saw in Filmer: namely, the problem of accounting for monarchy as the form of government growing out of *parental* power. Against Filmer, Locke had protested repeatedly that the honor and obedience due to a father was due also to a mother. Both reason and revelation insisted upon this, he claimed. If "paternal" power actually meant "parental" power, that is, the power of father *and* mother, why should the form of government growing out of that power be the government of one, not two? By the time Locke had reached this stage of his discourse, he had apparently abandoned the earlier line of argument, just as he had abandoned the usage of "parental" for "paternal." The father as "abler and stronger" would rule.

In this discussion, Locke was willing to concede the historical or anthropological case for patriarchalism. He was not ready to concede the moral case, however. Filmer had tied his moral and historical arguments together by using the Book of Genesis as the source of both. Locke split the two cases apart. Locke's biblical criticisms were intended to demonstrate the weakness of the moral conclusions which Filmer had drawn from the Genesis creation account. Thus, at best, Filmer was left with only an historical case. But, Locke insisted, history was not the source of morality. He wrote that "an Argument from what has been, to what should of right be, has no great force" (T.T., II, 103). Instead, he broke with history and based his moral theory on a new understanding of human nature. In doing so, however, he reopened questions closed by Filmer's theory. Locke had to deal with the political roles and status of women, children and servants. He was somewhat sensitive to Filmer's criticisms concerning the place of these politically unfit groups within contract theory. He certainly tried to make a consistent explanation of the relationship of children to civil society: "We are *born Free,* as we are born Rational; not that we have actually the Exercise of either: Age that brings one brings with it the other too. And thus we see how natural *Freedom and Subjection to Parents* may consist together and are both founded on the same Principle" (T.T., II, 61). No immature child could be expected to take part in the social compact. Yet children's inability to participate in politics would not preclude their right to consent to government when they reached adulthood. Locke indicated the necessity of each person giving consent as a condition of full political rights and full political obligation. Grown sons were free to make their own contract as were their fathers before them. An individual could not be bound by the consent of others but had to make a personal commitment through some separate act of consent.

But what of women? Unlike Tyrrell and Sidney, Locke remained silent on the question of their participation in the founding of political society. Of course, it is possible Locke referred to the role of women in the lost section of the *Treatises*. Or, perhaps Locke understood that explicit exclusion of women seriously weakened a theory grounded in the natural freedom of mankind. Yet Locke was also a good enough propagandist to have realized how deeply ingrained patriarchalism was in everyday life. Locke had criticized Filmer's use of the fifth commandment—"Honor thy father"—as a basis for political obligation. If the command were taken seriously, he charged, then "every Father must necessarily have Political Dominion, and there will be as many Sovereigns as there are Fathers" (T.T., I, 65). But the audience Locke was addressing was essentially an audience of fathers, household heads and family sovereigns. Locke had freed them from political subjection to a patriarchal superior—the king. He did not risk alienating his audience by clearly conferring a new political status on their subordinates under the patriarchal system, that is, on women. Nevertheless, in the absence of any sustained analysis of the problem of women, we may draw some conclusions from an examination of Locke's scattered thoughts on women.

Though Locke gave the husband ultimate authority within conjugal society, this authority was limited and non-political. Yet when Locke's account of the husband's conjugal authority was combined with his account of the historical development of political society, several questions occur which were never adequately resolved in Locke's moral theory. Did not the award of final decision-making power to the father and husband (in conjugal society) result in a transformation of "parental power" into "paternal power"? Was the subsequent development of political power based on paternal power a result of that transformation? What was woman's role in the establishment of the first political society? Since her husband was to be permitted final decisions in matters of their common interest and property, and since political society, obviously, was a matter of common interest, would her voice simply be "concluded" in that of her husband? If so, then Filmer's question recurs—what became of her rights as a free individual? Did she lose her political potential because she was deemed not as "able and strong" as her husband? If this were the case, Locke would have had to introduce new qualifications for political life.

Locke portrayed political society as an association of free, equal, rational individuals who were capable of owning property.[24] These individuals came together freely, since none had any power or jurisdiction over others. They agreed to form a civil society vested with power to legislative over life and death, and to execute its decisions in order to protect the vital interests of its members, that is, their lives, liberties and estates. Yet John Locke was certainly no believer in the absolute equality of human beings. Indeed, on that score, he was emphatic:

> Though I have said . . . *That all Men by Nature are equal,* I cannot be supposed to understand all sorts of *Equality; Age* or *Virtue* may give Men a just Precedency: *Excellence of Parts and Merit* may place others above the Common Level; *Birth* may subject some and *Alliance* or *Benefits* others, to pay an Observance to those whom Nature, Gratitude, or other Respects may have made it due (T.T., II, 54)

But these inequalities in no way affect an individual's basic freedom or political capacity, for Locke continued in the same passage:

> . . . yet all this consists with the *Equality* which all Men are in, in respect of Jurisdiction or Dominion one over another, which was the *Equality* I there spoke of, as proper to the Business in hand, being that *equal Right* every Man hath, *to his Natural Freedom,* without being subjected to the Will or Authority of any other Man (T.T., II, 54).

If "Man" is used as a generic term, then woman's natural freedom and equality could not be alienated without her consent. Perhaps a marriage contract might be taken for consent, but this is a dubious proposition. Locke had indicated that a marriage contract in no way altered the political capacity of a queen regnant (T.T., I, 47). While decision-making power over the common interests of a conjugal unit belonged to the husband, Locke admitted that the wife might have interests apart from their shared interests. Women could own separate property not subject to their husbands' control. If a husband forfeited his life or property as a result of conquest, his conquerors acquired no title to his wife's life or property.

Did these capacities entitle women to a political role? Locke never directly confronted the question; nevertheless, it is possible to compare Locke's qualifications for political life with his views of women. Locke used the Genesis account to show that women possessed the same natural freedom and equality as men. Whatever limitations had been placed on women after the Fall could conceivably be overcome through individual effort or scientific advance. Furthermore, women were capable of earning through their own labor, of owning property and of making contracts.

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[24] See MacPherson, *Possessive Individualism*, Ch. 5. MacPherson argues that Locke assumed a class differential in the distribution of these qualities. Full membership in political society would be limited to those who fully demonstrated them. The question under consideration here is the extent to which this class differential might also be a sex differential.

Locke and the Rational Woman. The one remaining qualification for political life is rationality. For Locke's views on the rationality of women it will be necessary to turn to his other writings, notably his *Thoughts on Education*.

In the published version of his advice on education, Locke mentioned that the work had been originally intended for the education of boys; but he added that it could be used as a guide for raising children of either sex. He noted that "where difference of sex requires different Treatment, 'twill be no hard Matter to distinguish."[25]

The *Education* was first written as a series of letters to Locke's friend, Edward Clarke, concerning the upbringing of Clarke's eldest son. Locke's intent was to provide a guide for the education of a gentleman. His attention was not focused directly on a lady's education, although later he offered the Clarkes guidance on the education of their daughter. From the latter advice we may discover just where Locke thought "difference of sex required different treatment."

Locke felt that his advice concerning a gentleman's education would have to be changed somewhat to fit the needs of Clarke's daughter. However, in a letter to Mrs. Clarke, Locke tried to convince her that his prescriptions were appropriate for girls and not unnecessarily harsh.[26] On the whole, Locke believed that except for "making a little allowance for beauty and some few other considerations of the s[ex], the manner of breeding of boys and girls, especially in the younger years, I imagine, should be the same."[27]

The differences which Locke thought should obtain in the education of men and of women amounted to only slight differences in physical training. While Locke thought that "meat, drink and lodging and clothing should be ordered after the same manner for the girls as for the boys," he did introduce a few caveats aimed at protecting the girls' complexions.[28]

Locke introduced far fewer restrictions in his plan for a young lady's mental development. In a letter to Mrs. Clarke he wrote: "Since, therefore I acknowledge no difference of sex in your mind relating . . . to truth, virtue, and obedience, I think well to have no thing altered in it from what is [writ for the son]."[29]

Far from advocating a special, separate and distinct form of education for girls, Locke proposed that the gentleman's education should more closely resemble that of young ladies. For example, he favored the education of children at home by tutors. Modern languages learned through conversation should replace rote memorization of classical grammars. In addition, Locke suggested that young gentlemen as well as young ladies might profit from a dancing master's instruction.

Taken as a whole, Locke's thoughts on education clearly suggest a belief that men and women could be schooled in the use of reason. The minds of both men and women were blank slates to be written on by experience. Women had intellectual potential which could be developed to a high level.

Locke's educational process was designed to equip young men for lives as gentlemen. Since the gentleman's life certainly included political activity, a young man's education had to prepare him for political life. If a young lady were to receive the same education, it should be expected that she, too, would be capable of political activity.

Locke's personal relations also may have reinforced his view of female rationality. Though he was generally secretive about his personal life, Locke did have a well-known close relationship with Damaris Cudworth Masham. She was a brilliant woman, a "theologian and correspondent of the intellectuals of her day," and perhaps, as Laslett suggests, "the first bluestocking of them all."[30] Locke described Damaris to a friend in glowing terms:

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[25] John Locke, *Some Thoughts Concerning Education*, sec. 6; also, see Locke to Mrs. Clarke, Jan. 7, 1683/4, in *The Correspondence of John Locke and Edward Clarke*, ed. Benjamin Rand (Cambridge: Harvard University Press, 1927).

[26] Locke to Mrs. Clarke, Jan. 7, 1683/4, in Rand, p. 121.

[27] Locke to Clarke, Jan. 1, 1685, in Rand, p. 121.

[28] Locke to Mrs. Clarke, in Rand, p. 103.

[29] Locke to Mrs. Clarke in Rand, pp. 102–03; while Locke admitted no difference between the sexes in their ability to grasp truth, he did realize that women had less practice in using that ability. He asked a friend to help him revise a Latin text of the *Essay Concerning Human Understanding*. Locke wanted assistance in "paring off superfluous repetitions . . . left in for the sake of illiterate Men and the softer Sex, not used to abstract Notions and Reasonings." Locke to William Molyneux, Apr. 26, 1695, in A. Bettesworth and C. Hitch, *Some Familiar Letters between Mr. Locke and Several of His Friends*, 3rd ed. (London, 1837), p. 88.

> The lady herself is so well versed in theological and philosophical studies and of such an original mind that you will not find many men to whom she is not superior in wealth of knowledge and ability to profit by it. Her judgment is excellent, and I know few who can bring such clearness of thought to bear upon the most abstruse subjects, or such capacity for searching through and solving the difficulties of questions beyond the range, I do not say of most women, but even of most learned men.[31]

What effects did Locke's relationship with Damaris Masham have on his views about women? Obviously this is a highly speculative matter. Locke had met Damaris some time before he began to advise Mrs. Clarke on the upbringing of her daughter. Damaris herself championed the cause of female education. In one of her (anonymously) published works, she castigated those English gentlemen, who, destitute of knowledge themselves, derived a sense of superiority from depriving women of knowledge. She realized that learned ladies risked becoming "a subject of ridicule . . . and aversion." Only a few "vertuous and rational persons" would rise above the conventional prejudice.[32] Surely Damaris Masham believed John Locke was among these exceptional individuals.

Locke wrote the *Treatises* before he met Damaris. While it is tempting to suggest that his relationship with Damaris caused him to move away from the somewhat more patriarchal sentiments of the *Second Treatise* toward the ideas of greater sexual equality found in his thoughts on education, to claim this would be to claim more than the evidence warrants. Locke's various views on women as expressed in the *Treatises* and his educational writings are not really incompatible. At most, we can maintain that Locke's acquaintance with Lady Masham reinforced his views on women—views already heavily influenced by his belief in individualism.

In summary, it does appear that Locke was a part of a shifting collective consciousness which made the sexual revolution a possibility. He did not totally reject all forms of patriarchalism but in many respects his thoughts on women anticipated such noted feminists as John Stuart Mill.

Locke did hold that there might be a foundation in nature for the subjection of wives to husbands. The pain of childbirth, the duration and frequency of pregnancy and the long dependency of the children helped dictate the form of conjugal society. In conjugal society, the final decision-making power belonged to the husband as the "abler and stronger." Locke was certainly not interested in abolishing what many feminists today see as the chief institution of modern patriarchy—the family. The form of conjugal society among human beings grew out of peculiarities in the human life cycle and in human nature. Human beings matured slowly and were acquisitive. These two factors shaped the character of the human family. Nevertheless, family relations—parent-child, and husband-wife—were not immutable or indissoluble as patriarchal theorists had claimed. The conjugal relationship was based on consent and the terms of the conjugal contract could be varied by the uniting couple. There was no inherent reason why the relationship had to be lifelong. Wives could have a right to separate from their husbands. When separations occurred, child custody could be granted to either parent.

Locke's views on women, as noted above, exemplified his individualism. While he believed that women did suffer from some natural weaknesses, he had a classic liberal faith in the ability of individual women to overcome these natural obstacles. The program of education he designed for young ladies attested to that faith and his own experience with Damaris Cudworth Masham confirmed it. Doubtless feminists today would find fault with the "concessions to female beauty" in his educational program. Yet his overall plan went far in breaking down traditional sex roles in education.

Locke believed that women shared the basic freedom and equality characteristic of all members of the species. Women were capable of rational thought; in addition, they could make contracts and acquire property. Thus it appeared that women were capable of satisfying Locke's requirements for political life. Yet Locke was never explicit about women's role in the formation of civil society. He gave no separate account of women's political obligation. He registered no protest over the rule of Queens Elizabeth or Anne or Mary; but he never stated whether he, like Tyrrell and Sidney, would have excluded the "whole multitude of women" from any form of political life.

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[30] Peter Laslett, "Masham of Otes: The Rise and Fall of an English Family," *History Today*, 3 (August, 1953), 535–43, at 536. See also Maurice Cranston, *John Locke: A Biography* (London: Longmans, Green, 1957).

[31] Locke to Limborch, Mar. 13, 1690/91, reprinted in H. R. Fox Bourne, *The Life of John Locke*, Vol. 2 (New York: Harper, 1876), pp. 212–13.

[32] [Damaris Cudworth Masham], *Occasional Thoughts in Reference to a Virtuous or Christian Life*, in George Ballard, *Memoirs of British Ladies* (London: T. Evans, 1775), pp. 262–69, at p. 267.

Finally, 300 years ago, Locke offered a "liberated" solution to a controversy which still rages in religious circles—the question of the fitness of women to act as ministers. In 1696 Locke, together with King William, attended a service led by a Quaker preacher, Rebecca Collier. He praised her work and encouraged her to continue in it, writing, "Women, indeed, had the honour first to publish the resurrection of the Lord of Love; why not again the resurrection of the Spirit of Love?"[33] It is interesting to compare Locke's attitude here with the famous remark made by Samuel Johnson on the same subject in the next century: "Sir, a woman's preaching is like a dog's walking on his hindlegs. It is not done well; but you are surprized to find it done at all."[34]

Perhaps a similar conclusion might be reached about the roots of feminism in Lockean liberalism. In a world where political anti-patriarchalism was still somewhat revolutionary, explicit statements of more far-reaching forms of anti-patriarchalism were almost unthinkable. Indeed, they would have been considered absurdities. Thus, while Filmer had presented a comprehensive and consistent patriarchal theory, many of his liberal opponents rejected political patriarchalism by insisting on the need for individual consent in political affairs but shied away from tampering with patriarchal attitudes where women were concerned. John Locke was something of an exception to this rule. Though his feminist sympathies certainly did not approach the feminism of Mill writing nearly two centuries later, in view of the intense patriarchalism of seventeenth-century England, it should be surprising to find such views expressed at all.

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[33] Locke to Rebecca Collier, Nov. 21, 1696, reprinted in Fox Bourne, p. 453.

[34] E. L. McAdam and George Milne, eds., *A Johnson Reader* (New York: Pantheon Books, 1964), p. 464.

Separation of Powers and the Madisonian Model: A Reply to the Critics*

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This article critically examines the commonly held proposition that Madison advocated separation of powers as a means of thwarting majority rule, or, conversely, of protecting identifiable minority interests. Rather, Madison's chief purpose in advancing the doctrine of separation of powers—one which was shared by the majority of his contemporaries—was to prevent governmental tyranny whose characteristic feature was seen as arbitrary and capricious rule resulting in government of men, not of laws. Many modern critics' analyses of the Madisonian model (most notably, Burns, Dahl, and advocates of a responsible party system) are seriously deficient because they fail to take into account this dimension of the model. Madison's writings, principally in The Federalist, *as well as his remarks at the Philadelphia convention, clearly indicate that one of his central concerns was simultaneously to provide for protection against governmental tyranny and to guarantee popular control of government. This article examines in some detail certain critical aspects of this endeavor.*

Central to most assessments of the democratic character of our constitutional order is the doctrine of separation of powers. Many, if not most, students of the American system have accepted the proposition that separation of powers was intentionally fused into our system to thwart majority rule in one way or another.[1] Indeed, the most persistently advanced democratic "reforms" of our institutions call for extensive modification or elimination of our system of separated powers because of the barriers that such a separation seems to pose to the implementation of the majority will.[2]

While populists around the turn of the century—James Allen Smith being the most notable—were among the first to give currency to the notion that the Constitution is a "reactionary" or "undemocratic" document,[3] modern students going well beyond the populists' preoccupation with constitutional forms, have focused their attention on what is fashionably called the "Madisonian model"[4] and its underlying assumptions regarding the need, utility, and purpose of two of its most important elements, checks and balances and separation of powers. Their findings in one crucial respect are the same as those of the populists: through the constitutional mechanism of separation of powers, Madison sought to protect "certain minorities whose advantages of status, power, and wealth would, he thought, probably not be tolerated indefinitely by a constitutionally untrammeled majority."[5] In a similar vein, Eidelberg writes: "Madison wished to institute a system of checks and balances to preserve the Republic from the leveling spirit."[6] James

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[1] In addition to those works cited below a very partial listing of works which advance this thesis would be: David Spitz, *Democracy and the Challenge of Power* (New York: Columbia University Press, 1958); Robert A. Dahl and Charles E. Lindblom, *Politics, Economics, and Welfare* (New York: Harper, 1953); Richard Hofstadter, *The American Political Tradition* (New York: Knopf, 1948); Vernon L. Parrington, *Main Currents in American Thought,* 3 vols. (New York: Harcourt, Brace, 1927–30); and Henry Steele Commager, *Majority Rule and Minority Rights* (New York: Oxford University Press, 1943).

[2] The varied and numerous proposals for a more disciplined and responsible party system are the most notable. For an examination of these proposals see Austin Ranney, *The Doctrine of Responsible Party Government* (Urbana: University of Illinois Press, 1962) and Evron M. Kirkpatrick, "Toward a More Responsible Two-Party System: Political Science, Policy Science, or Pseudo-Science?" *American Political Science Review,* 65 (December 1971), pp. 965–90.

[3] James Allen Smith, *The Spirit of American Government* (New York: Macmillan, 1907).

[4] The term "Madisonian model," very much in vogue today, was first used by James MacGregor Burns in *The Deadlock of Democracy* (Englewood Cliffs, N.J.: Prentice-Hall, 1963). Part I of the book which deals with the obstacles to majority rule posed by the American political system is entitled, "The Madisonian Model."

[5] Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), p. 31.

[6] Paul Eidelberg, *The Philosophy of the American Constitution* (New York: Free Press, 1968), p. 153.

MacGregor Burns puts this conclusion even more forcefully:

> [Madison] was not content with a flimsy separation of power that lunging politicians could smash through like paper. He was calling for barricade after barricade against the thrust of a popular majority—and the ultimate and impassable barricade was a system of checks and balances that would use man's essential human nature—his interests, his passions, his ambitions, to control himself.... It was a stunning solution to the Framers' problem of checking the tyranny of the majority.[7]

Modern criticism, however, has gone beyond the populists, principally by pointing out that Madison, in his efforts to prevent tyrannical majorities from ruling, relied principally on institutional checks and balances while largely ignoring the critical role of social checks and balances in a pluralistic society. Dahl writes in his widely acclaimed *Preface to Democratic Theory:*

> The Madisonian argument exaggerates the importance, in preventing tyranny, of specified checks to governmental officials by other specified governmental officials; it underestimates the importance of inherent social checks and balances existing in every pluralistic society. Without these social checks and balances, it is doubtful that the intragovernmental checks on officials would in fact operate to prevent tyranny; with them, it is doubtful that all of the intragovernmental checks of the Madisonian system as it operates in the United States are necessary to prevent tyranny.[8]

And along these same lines Burns critically asks certain very crucial questions with which we shall have occasion to deal later.

> If, as Madison said, the first great protection against naked majority rule was the broader diversity of interests in a larger republic and hence the greater difficulty of concerning their "plans of oppression," why was not this enough? Why would not any popular majority representing such a variety of interests perforce become so broad and moderate in its goals as never to threaten any major or even minor or individual interest? Why was it necessary to have what Madison called "auxiliary precautions" of checks and balances built right into the frame of government?[9]

In sum, according to these critics, either Madison didn't or couldn't perceive the crucial role of social checks and balances, or he did perceive this role but persisted in his efforts to establish and justify even further unnecessary checks and balances to thwart majority rule.

My purpose here, quite simply, is to challenge these interpretations. This task is not an easy one, largely because the critics of the Madisonian model have been selective and partial in their elaboration of the model, especially with respect to the role and purpose of separation of powers. To put this otherwise, the Madisonian model is an intricate construct which attempts to realize and accommodate more than one crucial end value or goal. Yet, more frequently than not, the present-day practice has been to judge the model on the basis of the degree to which it accords with a single value, namely, political equality and its derivative majority rule. As we shall see, this practice not only distorts assessment of the model's democratic character, it also, and more importantly, excludes from our purview fundamental, normative considerations which better enable us to comprehend the model. At the very least a fuller examination of separation of powers and its place in the Madisonian model would seem prudent before advancing widescale reform of our present institutions and practices.

With this end in mind, we shall examine the Madisonian model from three perspectives: (1) the purpose of separation of powers; (2) the compatibility between separation of powers and majority rule; and (3) the purposes of bicameralism (an integral component of Madison's separation of powers system) and their compatibility with majority rule. Throughout this analysis, I will deal with the contentions raised by Madison's critics, principally those of Dahl and Burns.[10]

Separation of Powers

"The accumulation of all powers legislative, executive and judiciary, in the same hands, whether of one, a few or many, and whether hereditary, self-appointed, or elective," Madison wrote in Federalist 47, "may justly be pronounced the very definition of tyranny." Of this proposition he went so far as to say, "No political truth is certainly of greater intrinsic value, or is stamped with the authority of more enlightened patrons of liberty. . . ."[11]

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[7] Burns, pp. 20–21.

[8] Dahl, p. 22.

[9] Burns, p. 21. To my knowledge this line of argument was first set forth by E. E. Schattschneider in his *Party Government* (New York: Farrar and Rinehart, 1942), "Madison's defense of federalism [his presentation in Federalist 10] annihilates his defense of the separation of powers. If the multiplicity of interests in a large republic makes tyrannical majorities impossible, the principal theoretical prop of the separation of powers has been demolished" (p. 9). As we shall see, however, Madison does not introduce separation of powers as a device to check factious majorities.

[10] Most of what follows also critically bears upon salient aspects of the thesis advanced by Paul Eidelberg in *The Philosophy of the American Constitution.*

Not only do the records of the Philadelphia Convention seem to bear out this assessment, the political writings of such figures as Adams and Jefferson, whose theories were otherwise markedly different, also reveal fundamental agreement on the proposition that accumulation of powers and tyranny were inseparable.[12] Moreover, as Madison correctly noted in Federalist 47, all but two of the states during the revolutionary era had attempted, albeit with very limited success, to provide in one fashion or another for separation of powers. In fact, in six of these state constitutions, we even find specific declarations to this effect.[13] Beyond this, despite the in-built provisions in the Constitution designed to insure separation of powers, three of the last four of the original 13 states to ratify the Constitution (Virginia, North Carolina, and Rhode Island) submitted recommendatory amendments designed, so it would seem, to reinforce this principle. The wording in each case was the same: "the legislative, executive, and judiciary powers of Government should be separate and distinct. . . ."[14]

While there did seem to be virtual unanimity on the need for separation of powers, the convention debates, as well as the state constitutions, reveal marked differences concerning the specifics of its implementation.[15] What is crucial to note, however, is that the Framers (and Madison as well) had consciously divorced the concept of separation of powers from that of the "mixed regime" with which it had been historically associated.[16] That is, the Framers retained essential elements of Montesquieu's teachings regarding the principle of separation of branches, but rejected the idea that the branches should represent the dominant social "classes" such as the democratic, aristocratic, and monarchical. In large part, this rejection was simply dictated by the realities of American society which did not possess the social divisions appropriate for a mixed regime as envisioned by Montesquieu. Charles Pinckney, among others, stressed this very point at the Philadeophial Convention:

> [the United] States contain but one order that can be assimilated to the British Nation, this is the order of Commons. They will not surely then attempt to form a Government consisting of three branches, two of which shall have nothing to represent. They will not have an Executive and Senate [hereditary] because the King and Lords of England are so. The same reasons do not exist and therefore the same provisions are not necessary.[17]

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[11] Jacob E. Cooke, ed., *The Federalist* (New York: Meridian, 1961), p. 324. All subsequent citations to *The Federalist* are from this edition.

[12] Jefferson, for example, was most critical of the Virginia Constitution of 1776 precisely because the powers of government were concentrated. In his oft-quoted words, "All the power of government, legislative, executive, and judiciary, result to the legislative body. The concentrating of these in the same hands is precisely the definition of despotic government. It will be no alleviation that these powers will be exercised by a plurality of hands and not by a single one. One hundred and seventy-three despots would surely be as oppressive as one." [William Peden, ed., *Notes on the State of Virginia* (New York: Norton, 1954), p. 120.] It is also interesting to note that both of Jefferson's drafts of a constitution for the state of Virginia (1776 and 1783) contain specific provisions for separation of powers.

John Adams, unlike Jefferson, can be viewed as a proponent of a mixed or balanced government wherein distinct classes would be represented, each with a veto over proposed legislation. See his *A Defense of the Constitution of Government of the United States of America* in *The Works of John Adams,* ed. Charles Francis Adams (Boston: Little Brown, 1850–56), Vols. 4 and 5. It should be pointed out that Adams' views never gained currency because the social structure of the United States, very dissimilar from that of England, was not amenable to such a balanced government. On this point see M. J. C. Vile, *Constitutionalism and the Separation of Powers* (Oxford: Clarendon Press, 1967), pp. 148–51. Also see our discussion below.

[13] These states were Georgia, Massachusetts, Virginia, Maryland, New Hampshire, and North Carolina. See Francis N. Thorpe, comp., *The Federal and State Constitutions,* 7 vols. (Washington, D.C.: Government Printing Office, 1909).

[14] Charles C. Tansill, ed., *Documents Illustrative of the Formation of the Union of the American States* (Washington, D.C.: Government Printing Office, 1927) at pp. 1028–29, 1045, and 1053.

[15] On the difficulties surrounding implementation of the doctrine, particularly with respect to the legislative and judicial branches see Edward S. Corwin, "The Progress of Constitutional Theory Between the Declaration of Independence and the Meeting of the Philadelphia Convention," *The American Historical Review,* 30 (April 1925), pp. 511–36.

[16] For two excellent works dealing with the development of the doctrine of separation of powers see Vile, *Constitutionalism and the Separation of Powers,* and William B. Gwyn, *The Meaning of the Separation of Powers,* Tulane Studies in Political Science, Vol. 9 (New Orleans, La.: Tulane University Press, 1965).

[17] *Documents Illustrative,* p. 273. Because we lacked such social "orders," Patrick Henry argued that

Likewise, it is clear that the goal sought through separation of powers was the avoidance of governmental tyranny—a goal long associated with the concept of a division of powers. Aristotle, for instance, provides us with an early example of a perceived relationship between a union of powers and tyranny. Tyranny, a perverted form of "perfect monarchy," he associated with capricious and arbitrary government wherein all powers, as we conceive of them today, were vested in the hands of one. "No freeman," he wrote, "if he can escape from it, will endure such a government."[18]

John Locke expanded upon Aristotle's formulation and provided us with the more modern conception of governmental tyranny:

> It is a mistake to think this fault is proper only to monarchies; other forms of government are liable to it as well as that. For wherever the power that is put in any hands for the government of the people and the preservation of their properties is applied to other ends, and made use of to impoverish, harass, or subdue them to the arbitrary and irregular commands of those that have it, there it presently becomes tyranny, whether those that thus use it are one or many.[19]

The link between Locke's thought, as well as Aristotle's, and American thought during the period preceding the adoption of the Constitution is unmistakable. The *Essex Result* provides one of the most detailed elaborations on the necessity of a division of powers to avoid governmental tyranny.

> If the three powers are united, the government will be absolute, *whether these powers are in the hands of one or a large number.* The same party will be the legislator, accuser, judge and executioner; and what probability will an accused person have of an acquittal, however innocent he may be, when his judge will also be a party.[20]

Moreover, any union of two powers was viewed as producing the same effect. If the legislative and judicial powers were joined, the laws would be "uncertain," they would reflect on the "whims," "caprice," or "the prejudice of the judge." If the legislative and executive powers were united, "the security and protection of the subject would be a shadow—the executive power would make itself absolute, and the government end in a tyranny."[21]

That such a view of the matter was shared by Madison also seems beyond doubt. Quoting from Montesquieu's *Spirit of the Laws*, Madison in Federalist 47, the first essay devoted exclusively to separation of powers, endorses this very same line of reasoning:

> The reasons on which Montesquieu grounds his maxim are a further demonstration of his meaning. "When the legislative and executive powers are united in the same person or body" says he, "there can be no liberty, because apprehensions may arise lest *the same* monarch or senate should *enact* tyrannical laws, to *execute* them in a tyrannical manner." Again: "Were the power of judging joined with the legislative, the life and liberty of the subject would be exposed to arbitrary controul, for the *judge* would then be *the legislator.* Were it joined to the executive power, *the judge* might behave with all the violence of *an oppressor."* Some of these reasons are more fully explained in other passages; but briefly stated as they are here, they sufficiently establish the meaning which we have put on this celebrated maxim of this celebrated author.[22]

We may say, then, that the chief end sought through separation was avoidance of capricious and arbitrary government. The end, however, can be stated more precisely and positively. Article XXX of the Massachusetts Convention of 1780 in which we find the injunction that no branch shall exercise the functions of another concludes "to the end it may be a government of laws and not of men."[23]

We have before us three facts which bear directly upon the modern interpretations of the Madisonian model to which we have referred. First, Madison's reference to tyranny in Federalist 47 is to governmental tyranny. This tyranny, quite simply, involves those in positions of authority using their powers arbitrarily and capriciously to abuse the nongovernmental portion of society. In this situation, the conflict comes down to the governors versus the governed as distinct from the problem of majority factions oppressing minorities discussed in Federalist 10. As obvious as this may seem, one of the chief weaknesses of modern criticism of the

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any system of separation of powers would be ineffective. Jonathan Elliot, ed., *Debates on the Adoption of the Constitution*, rpt., Vol. 3 (New York: Lenox Hill, 1974), 165.

[18] Benjamin Jowett, trans., *Politics* [Bk. 4, Ch. 11] (New York: Modern Library, 1943), p. 189.

[19] *A Second Treatise of Civil Government* (Ch. 18, Sect. 201) (Chicago: Gateway edition, 1955), p. 169.

[20] Oscar and Mary Handlin, eds., *The Popular Sources of Political Authority: Documents on the Massachusetts Constitution of 1780* (Cambridge, Mass.: Harvard University Press, 1966), p. 327.

[21] Oscar and Mary Handlin, eds., *The Popular Sources of Political Authority*, pp. 337–38.

[22] *The Federalist*, pp. 326–27.

[23] *Popular Sources*, p. 448.

Madisonian model has been its failure to make this distinction regarding the source and kinds of tyranny.[24] Dahl, for instance, manages to lump Madison's concern with majority factions together with his treatment of the problem of governmental tyranny.[25] He does this by transforming Madison's explicit definition of tyranny in Federalist 47 (accumulation of all governmental powers) to conform with his own definition ("severe deprivation of a natural right")[26]—a definition best suited for an analysis of Federalist 10 and the problem of majority factions. Dahl then treats Madison's solutions to governmental tyranny as if they were solutions to majority tyranny as well.

The predictable result is that Dahl's analysis and presentation, not Madison's, is difficult to follow. For instance, the charge that Madison underestimated the importance of social checks and balances is manifestly false particularly if one looks to Federalist 10 and his "solution" to the problem of *majority* tyranny. In this essay Madison cites such factors as the multiplicity of interests, the mutual suspicions that inevitably arise between interests, and the probability that representatives will be men "who possess the most attractive merit, and the most diffusive and established characters" as barriers to majority tyranny. At no point in the essay, save possibly with his brief mention of federalism, does Madison mention institutional structures as barriers to majority rule, or conversely, as protectors of vested minorities. In sum, we do not find recourse to institutional barriers because Madison believed the social checks and balances inherent in the extended republic were an adequate protection against majority tyranny.[27] When we reach Federalist 47, however, Madison is obviously dealing with tyranny of a different order, namely, *governmental* tyranny. And because this tyranny is of a different order, his solution to the problem is markedly different and does concern itself to a great degree with the constitutional checks and balances to which his critics refer. But, given the nature of his concern—control of government or of those with governmental powers—it is impossible to see how he could have done otherwise.

This particular difficulty becomes more obvious when we turn to the major concern expressed by Burns, namely, if, as Madison indicates in Federalist 10, the diversity of interests is a sufficient guard against majority factions or majority tyranny, why then did Madison erect even further barricades against majority rule with a system of checks and balances?[28] Burns' question would be perfectly legitimate, save for the fact that Madison was worried about two sources of tyranny, majority tyranny and, as I have shown, governmental tyranny. Burns has, along with Dahl, assumed that Madison's purpose in advocating a system of checks and balances was to thwart majority rule. But the assumption, as the text of *The Federalist* makes abundantly clear, is not warranted. Indeed, after the beginning two-thirds of Federalist 51, which deals with his solution to the problem of how to maintain the necessary partition of powers (in order to control, in Madison's words, "the abuses of government),[29] Madison changes focus and expressly identifies two distinct sources of tyranny. He writes: "*Second.* It is of great importance in a republic not only to guard the society against the oppression of its rulers [a matter which he has just finished treating of in the preceding part of this essay], but to guard one part of the society against the injustice of the other part [the problem of minority and majority faction]."[30] What follows is unmistakably a reiteration of his "extended republic" theory more elaborately set forth in Federalist 10. This even the most cursory reading reveals, as the following quote—the latter part of which is most interesting in light of the foregoing analysis—illustrates:

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[24] The distinction between governmental tyranny and majority tyranny which seems to be blurred or ignored by many moderns was not lost upon John C. Calhoun. Throughout his *Disquisition* he treats of these two sources of tyranny as distinct [*A Disquisition on Government* (1853; rpt. New York: Political Science Classics, 1947)].

[25] Dahl, cf. Ch. 1.

[26] Ibid., *Preface*, pp. 6–7.

[27] This particular confusion is reflected in the following passage from Dahl: "In retrospect, the logical and empirical deficiencies of Madison's own thought seem to have arisen in large part from his inability to reconcile two different goals. On the one hand, Madison substantially accepted the idea that all the adult citizens of a republic must be assigned equal rights, including the right to determine the general direction of government policy.... On the other hand, Madison wished to erect a political system that would guarantee the liberties of certain minorities whose advantages of status, power, and wealth would, he thought, probably not be tolerated indefinitely by a constitutionally untrammeled majority. Hence majorities had to be constitutionally inhibited" (p. 31). Clearly Madison in Federalist 10 did not see the need to constitutionally inhibit majorities. Nor is his concern in Federalists 47 to 51, which deal with separation of powers, the constitutional inhibition of majority factions. In these papers his evident concern is with guaranteeing the liberties of the people from arbitrary and capricious government (see text below).

[28] Cf. Burns, Ch. 1. Burns acknowledges that he is building upon Dahl's analysis (p. 345).

[29] *The Federalist*, p. 349.

[30] *The Federalist*, p. 351.

> In the extended republic of the United States, and among the great variety of interests, parties and sects which it embraces, a coalition of a majority of the whole society could seldom take place on any other principles than those of justice and the general good; and there being thus less danger to a minor from the will of the major party, there must be less pretext also, to provide for the security of the former, by introducing into the government a will not dependent on the latter; or in other words, a will independent of the society itself.[31]

A second point is this: Through separation of powers, Madison sought to avoid governmental tyranny which, as we have seen, is closely related to arbitrary and capricious government. We can, of course, conceive of situations where concentration of powers will not result in governmental tyranny—where, that is, those possessed of all governmental powers would not place themselves above the law or rule in their own private interests. So, it is fair to assume, could Madison. Yet Madison was concerned with fundamental principles on which to establish lasting constitutional procedures and forms. Therefore, we can readily imagine him to respond that such instances would be exceptional, so exceptional as to prove the rule. Or, we can also imagine his answer to have taken this form: "Over an extended period of time a concentration of power will inevitably result in governmental tyranny. Benevolent dictators and philosopher kings are hard to come by."[32]

Madison's definition of tyranny presented in Federalist 47 is not ambiguous or meaningless, except, perhaps, to the extreme positivist. Dahl's difficulty in this respect is, again, of his own doing. Having supplied Madison with a definition of tyranny ("severe deprivation of a natural right"), he finds tyranny to have "no operational meaning" and "Madison's own definition . . . a trivial one."[33] Whatever merit Dahl's analysis of the meaning of tyranny may have in relation to majority factions is not applicable to governmental tyranny. Madison sought to avoid capricious and arbitrary government which is characteristic of a government of men, not of laws.[34]

Finally, and most importantly for our subsequent analysis, what is apparent, not only from those portions of *The Federalist* which deal with separation of powers but also from the work as a whole, is Madison's conviction that separation of powers is a *necessary* (though not sufficient) condition for nontyrannical government. This, as we shall see, imposes a limitation on his theoretical development because other values and concerns have to be modified or reconciled with this basic requirement in mind.

Separation of Powers and Majority Rule

As emphasized above, critics of the Madisonian model have been quick to point out the incompatibility between separation of powers and majority rule. In this context the critics view separation of powers as a device to protect minorities of wealth, status, and the like, and, as such, a gross departure from the republican principle of political equality.

In retrospect it is obvious that Madison did have to deal with two incompatible goals, though from his vantage point he had no qualms about which goal should take precedence. But the incompatible goals were not, as some modern critics assert, majority rule and minority rights.[35] In Madison's mind, at least, these goals were not incompatible. The very first paragraph of Federalist 10, where he proposes a solution to the problem of majority factions, reveals this:

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[31] *The Federalist*, pp. 352–53. Notice that Madison, right after addressing himself to the issue of separation of powers, turns immediately to the problem of majority factions and declares this problem "solved" by social checks and balances which have nothing to do with constitutional checks and balances. At no point does separation of powers play a role in curbing majority factions.

[32] In this connection, it is highly doubtful that Madison was concerned with majority tyranny as that term is normally used. He was concerned about majority factions which could perform a tyrannous act. Madison does acknowledge at least the possibility of this occurring ("seldom" is his word), but this is quite different from a permanent condition of tyranny associated with a concentration of governmental powers. On these grounds majority tyranny (i.e., the act of a factious majority) is not on the same theoretical plane as governmental tyranny.

[33] Dahl, p. 24.

[34] *The Federalist*, pp. 326–27.

[35] Be they economic, property, social, or civil rights. The incompatibility cannot be couched in terms which would suggest that Madison believed in political equality but thought that some people were more equal than others. See Dahl, Ch. 1, p. 31.

> Among the numerous advantages promised by a well-constructed Union, none deserves to be more accurately developed than its tendency to break and control the violence of faction. The friend of popular government never finds himself so much alarmed for their character and fate, as when he contemplates their propensity to this dangerous vice. He will not fail therefore to set a due value on any plan which, *without violating the principles to which he is attached,* provides a proper cure for it.[36]

Madison proceeds then to set forth his famous theoretical "solution" to this problem by showing how the need for representation coupled with the multiplicity and diversity of interests in an extended republic by themselves provide "a Republican remedy for the diseases most incident to Republican Government."[37] Moreover, we repeat, his solution does not involve institutional or constitutional mechanisms.

At various points in *The Federalist* and other writings, Madison seems fully aware of pure republicanism and what the structure of a model republican government would involve. Why, then, did he support checks and balances which to all outward appearances would only serve to diffuse or dilute majority control and direction of government, essential elements of republicanism? In asking this question, perhaps the most important question relative to the criticisms directed as his model, we are focusing on one incompatibility of value goals which goes a long way toward understanding the model and the form it takes. The answer, in light of what we have said in the previous section, is this: adequate provision for separation of powers, necessary for a nontyrannical government, imposes demands which in certain particulars are at variance with the principles of pure republicanism.

Let us tend carefully to the nature of this incompatibility, lest it be understood to mean that separation of powers imposes requirements which were intended to prohibit majority rule and protect specific minorities, as the critics contend. We can do this best by specifically considering the problems associated with the legislative branch which, on all sides, is considered the mainspring of the republican principle and whose very bicameralism is viewed by contemporaries as *prima facie* evidence of the undemocratic character of the Madisonian model.

We cannot overestimate Madison's concern that the legislative body represented the greatest threat to separation of powers. On July 17th at the Philadelphia Convention, for instance, Madison set forth a theme that was to recur, principally in Federalists 48 to 50, where he discussed how to preserve the "necessary partition of powers."

> Experience had proved a tendency in our governments to throw all power into the Legislative vortex. The Executives of the States are in general little more than Cyphers; the legislatures omnipotent. If no effectual check be devised for restraining the instability of encroachments of the latter, a revolution of some kind or other would be inevitable. The preservation of Republican Government therefore required some expedient for the purpose, but required evidently at the same time that in devising it, the genuine principles of that form should be kept in view.[38]

This passage also reveals Madison's desire to maximize simultaneously two goals, republicanism and separation of powers.

Madison's concern on this score was, beyond any question, based on the fact that the legislative body, consonant with republican principles, would possess the vast bulk of the substantive powers of government. The problem, therefore, of guarding against a concentration of powers in a republican government, he perceived, differed from that encountered in "mixed" monarchies. "The founders of our republics," he writes in Federalist 48, have been so preoccupied with the "danger to liberty from the overgrown and all-grasping prerogative of an hereditary magistrate" that they have overlooked "legislative usurpations" which, in a republic, pose the greatest threats to liberty.[39]

Essays 48, 49, and 50 assume particular significance in understanding the answer which Madison eventually provides in essay 51. More importantly for our purposes, they deal with the question of what role a majority is to play in maintaining separation of powers which largely comes down to controlling the legislative branch vis-à-vis the other branches.

What, briefly, does Madison say in these essays? First, he contends in 48 that a constitutional provision to the effect that each department should stay within its boundary will definitely not suffice to preserve the necessary separation. Why? For several reasons. "Experience assure us, that the necessary efficacy of [such a] provision has been greatly over-rated; and some more adequate defence is indispensibly necessary for the more feeble against the more powerful members of the government."[40] And because of the number, nature, and importance of the powers vested in the legislature by the Constitution,

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[36] *The Federalist,* p. 56; emphasis added.

[37] Ibid., p. 65.

[38] *Documents Illustrative,* pp. 398–99.

[39] *The Federalist,* p. 333.

> it [the legislature] can with greater facility, mask under complication and indirect measures, the encroachments which it makes on the co-ordinate departments. It is not unfrequently a question of real nicety in legislative bodies, whether the operation of a particular measure, will, or will not extend beyond the legislative sphere. On the other side, the executive power being restrained within a narrower compass, and being more simple in its nature; and the judiciary being described by landmarks, still less uncertain, projects of usurpation by either of these departments, would immediately betray and defeat themselves.[41]

Furthermore, because the legislature "alone has access to the pockets of the people, and . . . a prevailing influence over the pecuniary rewards of those who fill the other departments, a dependence is thus created in the latter, which gives still greater facility to encroachments of the former."[42]

Second, in Federalist 49 Madison addresses himself to the question of whether occasional appeals initiated by two-thirds of the members of two of the three branches calling for a constitutional convention would serve to maintain the necessary separation of powers.[43] Again, he is something more than skeptical. Such occasional appeals, he argues, could be thwarted by a one-third minority in any two branches; they would carry with them the implication that the government is defective, thereby seriously undermining that popular veneration of government which is necessary for "stability"; and, what is more, they would serve to arouse the "public passions" to a dangerous degree. "But the greatest objection of all," writes Madison, "is that the decisions which would probably result from such appeals, would not answer the purpose of maintaining the constitutional equilibrium of the government." Judges and members of the executive branch are relatively few "and can be personally known to a small part only of the people." In contrast, the legislators are "numerous," "are distributed and dwell among the people at large," and have "connections of blood, of friendship and of acquaintance [which] embrace a great proportion of the most influential part of the society." Given these conditions, "it could hardly be supposed" that the judicial and the executive branches "would have an equal chance for a favorable" resolution of any conflict with the legislature.[44]

Finally, in Federalist 50, he considers whether periodic appeals to the people, at fixed intervals, would preserve the necessary partition. He also finds this proposal inadequate. If the interval between appeals is a short one, "the measures to be reviewed and rectified, will have been of recent date, and will be connected with all the circumstances which tend to viciate and pervert the result of occasional revisions." On the other hand, if the interval is a relatively long one, the "distant prospect of public censure" probably would not serve as an adequate restraint on the more immediate motives of that department bent on encroachment.[45] By way of illustrating his contentions regarding periodic appeals, he discusses the experience of Pennsylvania's "council of censors." This popularly elected council which met in 1783–84 was, as Madison observes, charged with the responsibility of determining "whether the Constitution [of Pennsylvania] had been violated and whether the legislature and executive departments had encroached on each other." The results of this procedure, he notes, were predictable and far from satisfactory: the men who were elected to judge of the actions of the legislative and executive branches were, to a great extent, those who had served in those very branches at the time the questionable actions were committed; party animosities in the council were so severe that "*passion*, not *reason*" seemed to dominate the proceedings; and the legislature felt no obligation to abide by the decisions of the council.[46]

What can be said of Madison's analysis relative to republicanism and separation of powers? The most striking fact is that, for reasons specified, Madison is of no mind to allow popular majorities to decide disputes between the branches of government. This mode of resolution, he felt, was certain to result in legislative tyranny because the experiences at the state level had shown that this branch would most likely commit the transgressions and then would, for all intents and purposes, act as the judge of its own actions.

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[40] *The Federalist*, p. 333.

[41] Ibid., p. 334.

[42] Ibid.

[43] In this Madison is addressing himself to a scheme for preserving the independence of the branches set forth by Jefferson in his *Notes on Virginia*.

[44] *The Federalist*, pp. 341–42.

[45] Ibid., p. 334.

[46] Ibid., pp. 345–46.

Thus, preservation of the partition of powers necessitated this departure from the republican principle.

More revealing, in light of the contentions advanced by some contemporary students, is the nature of this limitation. First, majority control is inhibited only with respect to resolution of disputes between the branches on matters involving their spheres of authority. This leaves popular majorities as free as the most ardent populist could wish to initiate and enact any social policy to their liking. In this regard, Madison's limitations scarcely touch at all upon curbing majority tyranny and then only by indirection.

Second, and equally important, Madison is not seeking to curb majority initiative. There is *no intimation* that a popular majority itself would push for proposals designed to obliterate the partition of powers.[47] On the contrary, it is the legislators themselves who, because they are operating as a small body in a context relatively free from the restraints inherent to an extended republic,[48] would press forward with such schemes and then, after the fact, seek popular endorsement. Thus, the limitation is not directed at curbing majorities in the usual sense of that term. And, in this connection, we cannot help but note Madison's total silence in these essays about separation of powers being in any way related to prevention of majority tyranny.

Finally, this very partial limitation on the sphere of majority decision making is consonant with a broader theory of constitutionalism which Madison and most of the Framers shared.[49] Within the confines of this theory, fundamental law or the Constitution could only be changed through an act as authoritative as the adoption of the Constitution itself (i.e., the amendment process). Separation of powers, inseparable as it is from the constitutional fabric, would certainly seem, on these grounds, to be immune from alteration or abolition by simple majorities. If, that is, popular majorities could legitimize basic alteration in the relative powers of the three branches, the way would be open to allowing basic constitutional alterations by simple legislative majorities.

The solution to the problem that Madison set out to resolve (how to maintain in practice the necessary partition of powers) is, as is well known, contained in Federalist 51. It "consists in giving to those who administer each department the necessary constitutional means and personal motives to resist encroachments of the others. The provision for defence must in this, as in all other cases, be made commensurate to the danger of attack."[50] This would seemingly obviate the need for frequent or periodical appeals to the people. And Madison in elaborating on this solution continues: "Ambition must be made to counteract ambition. The interest of the man must be connected with the constitutional rights of the place. It may be a reflection on human nature, that such devices should be necessary to controul the abuses of government. But what is government itself, but the greatest reflection on human nature?"[51]

While an evaluation of this solution is beyond our purposes here, two comments relative to it are in order.

1. Madison does rely upon constitutional or institutional mechanisms for maintenance of separation of powers. His reference to the "necessary constitutional means" in the context of the passage cited above is evidence of this. However, he is far from oblivious to the need for the proper type of internal motivation, a distinctly noninstitutional factor, to achieve this end. References to "personal motive," "ambition" counteracting "ambition," and the connection between the "interest of the man" and "the constitutional rights of the place" are abundant evidence to the effect. He may well have reasoned that proper constitutional provisions would unleash, protect, and even cultivate behavior patterns conducive to the end he sought.

Yet, and to a matter which has not been explored thoroughly to my knowledge, Madison must have presumed limits to the behavior he anticipated. His discussion, couched as it is in terms of conflict and competition, might well lead one to believe that such would be the normal state of affairs between the branches. But clearly, if this were the case, adoption of the model would be an open invitation to stalemate and catastrophe. For this reason, we can safely surmise that one unarticulated premise of the Madisonian system must have been that the members of the branches would hold substantially the same views regarding the legitimate domain of the three branches and that, moreover, these members would show a high degree of forbearance, high enough at any rate not to repeatedly push the system to the brink of collapse.

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[47] To this point he writes in Federalist 63, "The people can never wilfully betray their own interests: But they may possibly be betrayed by the representatives of the people; and the danger will be evidently greater where the whole legislative trust is lodged in the hands of one body of men, than where the concurrence of separate and dissimilar bodies is required in every public act" p. 426–27.

[48] In this regard, Madison writes in Federalist 48 that it is against a legislature "which is sufficiently numerous to feel all the passions which actuate a multitude; yet not so numerous as to be incapable of pursuing the objects of its passions, by means which reason prescribes . . . that the people ought to indulge all their jealousy and exhaust all their precautions," p. 334.

[49] See Federalist 78 where this theory is articulated with respect to judicial review.

[50] Federalist 78, p. 349.

[51] Ibid.

2. Madison's solution is, in his own words, directed to controlling the "abuses of government." He acknowledges at the end of this particular passage the more general maxim of republican government, "dependence on the people is no doubt the primary controul on government," but he goes on to say consistent with his teachings on this point that "experience has taught mankind the necessity of auxiliary precautions."[52] Clearly the phrase "auxiliary precautions" refers to additional obstacles to governmental abuses and not to majority tyranny.[53]

Bicameralism and Majority Rule

For critics of the Madisonian system, as we have mentioned, the bicameral structure of Congress seems to provide incontrovertible evidence of Madison's hostility toward republicanism and his desire to protect vested minorities. At first glance, the case is a strong one which, without injustice, we may put as follows: separation of powers does not in theory, at least, require a bicameral legislature. Indeed, a bicameral legislature is a significant deviation from the republican principle for it divides the purely democratic element of the constitution which can only serve to diffuse, blunt, or obfuscate the will of popular majorities. In fact, while separation of legislative, executive, and judicial powers does not inherently represent a departure from pure principles of republicanism, bicameralism does. Moreover, a second or "upper" chamber cannot help but develop a "clubbishness" or sense of superiority which will set it apart from the "lower" house and transform it into a more "aristocratic" body with views markedly different from those of the "lower" chamber.[54]

At face value, these and like contentions are deceptively convincing, the more so as a second chamber is historically associated with institutions representative of the aristocratic class.[55] Yet, in so far as the Madisonian model is concerned, such views of the second chamber are far from the mark and serve only to conceal an important theoretical dimension of Madison's theory.

We must, in treating of bicameralism and its purpose, once again bear in mind Madison's overriding concern to maintain separation of powers in order to avoid governmental tyranny. This, we may say, was his principal reason for advocating a divided legislature. So much we find in Federalist 51 where he writes of the impossibility of giving "each department an equal power of self-defence" because in republican governments "the legislative authority, necessarily, predominates." Therefore, he writes, the "weight of the legislative authority requires that it should be . . . divided."[56] In this assessment he was undoubtedly influenced by the fact that virtually all the major powers of government, consistent with the republican principle, were vested by the Constitution in Congress.

The need for two chambers to prevent governmental abuse was a recurrent theme in his thoughts and writings. In speaking to the Convention on 26 June he observed,

> A people deliberating in a temperate moment, and with the experience of other nations before them, on the plan of Government most likely to secure their happiness, would first be aware, that those charged with the public happiness,

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[52] Federalist 78, p. 348.

[53] Burns (p. 22) mistakenly reads this as "auxiliary precautions" against majority tyranny. The error is understandable in light of his basic confusion concerning the purpose of separation of powers.

[54] This we submit as a composite argument drawn not only from the critical assessments of the Madisonian model in terms of its democratic character but from legislative behavioral studies as well. In any event, bicameralism certainly opens up the possibility of deadlock and its very existence suggests that there can be two popular majorities on any given issue, a notion logically inconsistent with majoritarian theory.

[55] This, in one form or another, is what most of the critics of the Madisonian model contend bicameralism was intended to do. In this respect, they see the Framers (and Madison as well) attempting to create a "mixed government" along the lines advocated by John Adams. This interpretation was, no doubt, bolstered by Charles A. Beard's *An Economic Interpretation of the Constitution of the United States* (New York: Macmillan, 1935) which related structural forms to economic interests. See also Malcolm P. Sharp, "The Classical Doctrine of the Separation of Powers," *University of Chicago Law Review*, 2 (April 1935), pp. 385–436.

[56] *The Federalist*, p. 350.

> might betray their trust. An obvious precaution against this danger would be to divide the trust between different bodies of men, who might watch and check each other.[57]

A second chamber, he noted on this occasion, would serve "to protect the people against their rulers."

Again, in Federalist 62, when dealing specifically with the justifications for the Senate, he writes:

> *First.* It is a misfortune incident to republican government, though in a less degree than to other governments, that those who administer it, may forget their obligations to their constituents, and prove unfaithful to their important trust. In this point of view, a senate, as a second branch of the legislative assembly, distinct from, and dividing the power with, a first, must be in all cases a salutary check on the government. It doubles the security to the people, by requiring the concurrence of two distinct bodies in schemes of usurpation or perfidy, where the ambition or corruption of one, would otherwise be sufficient. This is a precaution founded on such clear principles, and now so well understood in the United States, that it would be more than superfluous to enlarge on it.[58]

Clearly we see that in Madison's estimation the requirement of bicameralism necessitated a deviation from the pure republican structure. And we can also see clearly that this particular deviation is thoroughly consistent with and best understood in terms of his broader theory concerning the need to avoid governmental tyranny, as well as his fear of legislative usurpation. Moreover, his support of bicameralism is quite removed from any concern with majority tyranny or minority "rights" and, on the positive side, may even be viewed as an effort to preserve conditions necessary for popular majorities to form and operate free from governmental control.

While his concern with preventing governmental tyranny would alone justify his defense of bicameralism, Madison offers still another reason for it which does bear upon the problem of majority tyranny. It is at this point that bicameralism fits hand in glove with his concern to prevent majority tyranny. Since this coincidence clearly bears upon the republican character of the Madisonian model, I shall examine it with some care.

In his June 26th remarks at Philadelphia, Madison notes that a second end to be served by bicameralism is "to protect the people against the transient impressions into which they themselves might be led." And, in Federalist 63, after surveying six advantages of bicameralism "only as they relate to the representatives of the people," he continues,

> To a people as little blinded by prejudice or corrupted by flattery as those whom I address, I shall not scruple to add, that such an institution may be sometimes necessary as a defence to the people against their own temporary errors and delusion. As the cool and deliberate sense of the community ought, and actually will in all free governments, ultimately prevail over the views of its rulers; so there are particular moments in public affairs, when the people stimulated by some irregular passion, or some illicit advantage, or misled by the artful misrepresentations of interested men, may call for measures which they themselves will afterwards be the most ready to lament and condemn. In these critical moments, how salutary will be the interference of some temperate and respectable body of citizens, in order to check the misguided career, and to suspend the blow meditated by the people themselves, until reason, justice and truth, can regain their authority over the public mind?[59]

As if by way of answering those, such as Burns, who ask why the "solution" provided in Federalist 10 is not a sufficient barrier to majority tyranny, Madison writes:

> It may be suggested, that a people spread over an extensive region, cannot like the crouded inhabitants of a small district, be subject to the infection of violent passions; or to the danger of combining in pursuit of unjust measures. I am far from denying that this is a distinction of peculiar importance. I have on the contrary endeavored in a former paper [Federalist 10], to shew that it is one of the principal recommendations of a confederated republic. At the same time, this advantage ought not to be considered as superseding the use of auxiliary precautions.[60]

In the framework of Madison's thought, this "auxiliary precaution" is, so to speak, a "bonus" derived from his perceived need to protect the people from their government through bicameralism. Put otherwise, though needed to guard against governmental tyranny, the second chamber also provides a further "auxiliary precaution" against majorities. In these terms, which coincide with the manner in which Madison presents his theory, we must seriously question what seems to be a commonly held presumption to the effect that a second chamber was designed primarily to prevent the so-called excesses of democracy and to protect vested minorities. Rather, to Madison's way of thinking a second chamber was necessary, quite apart from any role it might play with respect to the problem of majority tyranny and minority "rights."

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[57] *Documents Illustrative,* p. 279.

[58] *The Federalist,* p. 418.

[59] *The Federalist,* pp. 424–25.

[60] Ibid., p. 425.

We gain a further insight into this relationship between republicanism and bicameralism by examining the function the Senate is to perform in curbing majorities. Here the only deviation, if it can fairly be called that, from the republican principle relates to a matter of delay in responding to the wishes of a popular majority until such time as "reason, justice and truth" can "regain their authority over the public mind." He seeks to insure that "the cool and deliberate sense of the community" will predominate—an end which, we may surmise, was primarily fostered by conditions associated with an extended republic, which likewise necessitated delay in the process of the formation of majorities.

Moreover, if we look to the factors which contribute to the Senate's effectiveness in performing this delay function, we see that they are fundamentally noninstitutional. In his speech of June 26th which bears directly upon majority factions and the role of a second chamber, Madison inquires, "How is the danger in all cases of interested coalitions to oppress the minority to be guarded against?" His answer: "Among other means by the establishment of a body in the Government sufficiently respectable for its wisdom and virtue, to aid on such emergencies, the preponderance of justice by throwing its weight into that scale."[61]

The Senate would serve as an institution composed of a "temperate and respectable body of citizens" who would, because of their "wisdom and virtue" be capable of cultivating those "internal restraints" so necessary for the avoidance of majority tyranny. Failing this, the Senate would pose no difficulties for persistent majorities. In Madison's words, there is an "irresistible force possessed by that branch of a free government, which has the people on its side." He continues:

> Against the force of the immediate representatives of the people, nothing will be able to maintain even the constitutional authority of the Senate, but such a display of enlightened policy, and attachment to the public good, as will divide with that branch of the legislature, the affections and support of the entire body of the people themselves.[62]

Equally important, Madison does not urge a second house with in-built or entrenched minorities. Quite the contrary. Madison was most critical of the Connecticut compromise, principally on grounds that it did deviate from the republican principle. His exchange with Paterson at the Convention on July 9th indicates this. Paterson in discussing "the true principle of Representation" calls it "an expedient by which an assembly of certain individuals chosen by the people is substituted in place of the inconvenient meeting of the people themselves." Madison, in response to this, "reminded Mr. Patterson that his doctrine of Representation which was in its principle the genuine one, must for ever silence the pretensions of the small States to an equality of votes with the large ones. They ought to vote in the same proportion in which their citizens would do, if the people of all the States were collectively met."[63]

By all evidence, Madison had to "swallow hard" in accepting the Connecticut compromise. In Federalist 62, he remarks that the compromise "is allowed on all hands to be the result not of theory, but 'of a spirit of amity, and that mutual deference and concession which the peculiarity of our political situation rendered indispensable.' "[64] But the proposed constitution, even with provision for equality of state representation in the Senate was, in his judgment, a "lesser evil" than the existing form of government under the Articles.

More to the point—save for southern sectional interests—Madison could not conceive of any interest in need of a structural protection in the Senate which might necessitate a deviation from the republican principle in either the mode of election or representation:

> He admitted that every peculiar interest whether in any class of citizens, or any description of States, ought to be secured as far as possible. Wherever there is danger of attack, there ought be given a constitutional power of defence. But he contended that the States were divided into different interests not by their difference of size, but by other circumstance; the most material of which resulted partly from climate, but principally from the effects of their having or not having slaves.[65]

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[61] *Documents Illustrative*, p. 281.

[62] *The Federalist*, p. 431.

[63] *Documents Illustrative*, p. 345.

[64] *The Federalist*, p. 416.

[65] *Documents Illustrative*, p. 310.

Thus, he could write of the Connecticut compromise in Federalist 62:

> It must be acknowledged that this complicated check on legislation may in some instances be injurious as well as beneficial; and that the peculiar defence which it involves in favour of the smaller State, would be more rational, if any interests common to them, and distinct from those of the other States, would otherwise be exposed to a peculiar danger.[66]

Yet, Madison's very line of reasoning on this point could be used to argue that the Connecticut compromise, though a deviation from republicanism, did not, in fact, entrench specific minorities. If, that is, no great dissimilarities of interests were to be found between the small and large states, there is no reason, a priori, to assume that this body would act to protect any specific minority or to thwart legitimate majorities. Perhaps for this reason, Madison could write in good conscience, "it is not impossible that this part of the Constitution may be more convenient in practice than it appears in contemplation."[67]

Conclusion

Though the foregoing analysis of separation of powers and its role in the Madisonian model opens upon new avenues for an exploration of the intended roles of the three branches in our constitutional system,[68] I shall make just two general observations which are directly related to this specific undertaking.

First, intelligent and meaningful evaluation of the Madisonian model makes sense only to the extent that we "enter" the model itself. This is true with respect to a full understanding of the values which Madison sought to maximize, the difficulties he encountered in this enterprise, as well as evaluation of the model in terms of principles or norms such as those associated with liberal democracy. For instance, as this paper illustrates, in Madison's framework of thought there was little to fear from Congress, acting in the capacity of a *translator* of a deliberate majority will. Nor did he feel that the Congress as constituted would be able to thwart persistent and, thus presumably, mature and deliberate majorities. What he did fear was Congress, using its enormous powers and prestige, acting as a force independent of society and imposing its will—much in the fashion of elected despots who presume to articulate the "general will"—on the whole society. Bearing this in mind, we can easily see that from Madison's point of view a more pristine form of republicanism, such as that advanced by his modern critics, would carry with it the probability of governmental tyranny.

We can state this another way: without effective separation of powers any system of government, even one in which republican values (i.e., political equality and majority rule) are fully realized, will degenerate into tyranny. The problem of governmental tyranny is, then, common to all forms of government. Madison most certainly must have subscribed to these or basically similar propositions. And, given this perception of the matter, we can scarcely fault him for his advocacy of separation of powers. But Madison's concerns went well beyond this; he had to "reconcile" the need for separation of powers with the principles of republicanism. He can, of course, be faulted for the form his reconciliation finally assumed, but one would have to deny the existence of governmental tyranny in order to ignore the problems it poses, particularly in the deliberate construction of a republican form of government.

This leads to a second general and related observation. One of the great hindrances to getting "inside" the Madisonian model, so it would seem, has been a preoccupation with "one value" analysis.[69] Such analyses, because they do not grasp the inner dimensions and tensions of the model, ultimately come down to a critique of the model without appreciation or consideration of the fundamental values upon which it rests, no matter how sensible or worthy they may be. Specifically, Madison's alleged theoretical shortcomings almost invariably relate to deviations from the principles of "liberal democracy," political equality and its corollary majority rule.[70] Principally on this score, Dahl concludes that Madison's theory will not bear up under examination and could with relative ease be placed "in the camp of the great antidemocratic theorists," such as Plato and Lenin. More: his theory centers "on the goal of avoiding majority control" and "goes about as far as . . . possible" in this direction "while still remaining within the rubric of democracy."[71]

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[66] *The Federalist,* p. 417.

[67] Ibid. Madison's "wish" seems to have been realized. Writes Robert A. Dahl, "The conclusion seems inevitable that the benefits and disadvantages flowing from equal state representation in the Senate are allocated in an entirely arbitrary fashion and cannot be shown to follow from any general principle." *Pluralistic Democracy in the United States: Conflict and Consent* (Chicago: Rand McNally, 1967), p. 125.

[68] For a pioneering effort in this direction see Martin Diamond, "Conservatives, Liberals, and the Constitution," in *Left, Right and Center,* ed. Robert A. Goldwin (Chicago: Rand McNally, 1969), pp. 60–86.

[69] I have borrowed this term from Professor Charles S. Hyneman.

[70] It is true that the model is also attacked on grounds that its structural forms prevent the realization of economic and social "democracy." But this is usually viewed as the very object of its presumed deviation from democratic principles.

Clearly, however, Madison's deviations from political equality, as we have seen, were trivial, the more so when compared with those attributed to him by his critics. More importantly, they were mandated by what Madison perceived to be a requisite for a republic without governmental tyranny. Indeed, the degree to which he was able simultaneously to maximize both the goals of republicanism and the necessary partition of powers is astonishing. Of course, one could say that Madison's concern with governmental tyranny was unfounded, exaggerated, or that the means he adopted to the end were inappropriate. Nevertheless, I submit, no sensible person immersed in the better part of the Western liberal tradition would ignore the potential danger of governmental tyranny. For this reason, if no other, Madison's concern still remains a challenge to the modern political theorist.

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[71] Dahl, *Preface*, p. 32.

Representativeness and Elections: A Policy Analysis*

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This paper looks at representativeness in terms of policy dimensions. As previous studies also have demonstrated, the analysis provides evidence that the level of policy agreement varies across policy domains. The policy linkage is weak on questions of taxation and government administration. Only on contemporary liberalism does the level of policy agreement begin to meet the standards of representational theory. The occurrence of elections is found to be a particularly important determinant of legislative voting vis-à-vis constituency preferences. California senators, elected for four-year terms, are undeniably unrepresentative to direct opinion during the first three years of their terms; representativeness increases dramatically during the final year. Senators facing the possibility of having their reelection fortunes adversely affected by a presidential campaign are particularly attuned to district opinion during the final year of their terms. Assemblymen, who are elected for two-year terms, are attuned to the wishes of their constituencies throughout their terms. As the Founding Fathers believed, the frequency of elections conditions representatives' loyalty to the preferences of the represented.

Democratic theorists long have debated the question of whether the preferences and opinions of the represented *ought* to be given due consideration in the councils of their representatives.[1] This paper examines the extent to which such preferences and opinions *are* given consideration. Specifically, we shall examine the relationship between representatives' roll-call votes and their constituencies' preferences in the institutional setting of the California state legislature. Since elections are at the core of the representative process, we will give particular attention to the way in which the temporal proximity of elections specifies the level of policy agreement between representatives and their constituencies.

Two analytical approaches have been used in the study described in this paper; (1) comparisons of policy agreement between the lower and upper chambers, and (2) a modified longitudinal analysis, spanning the period 1970–73.

Past Findings

Aided by advances in data processing technology, research on the extent of policy agreement between representative and constituency has proliferated. Although contradictory findings continue to limit our ability to make general statements, there are at least three aspects of the representative process on which there now appears to be some meaningful consensus.[2]

1. The overall importance of constituency as a determinant of legislators' voting behavior is limited.[3]
2. Policy agreement varies across issues; constituency preferences pertaining to individual rights are given more con-

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[1] The normative questions of representation are treated in Hanna F. Pitkin, ed., *Representation* (New York: Atherton, 1969).

[2] The most current and comprehensive summary of past findings can be found in Morris P. Fiorina, *Representatives, Roll Calls, and Constituencies* (Lexington: D.C. Heath, 1974), especially Ch. 1. See also Heinz Eulau and Katherine Hinckley, "Legislative Institutions and Process," in *Political Science Annual*, Vol. 1, ed. James A. Robinson (Indianapolis: Bobbs-Merrill, 1966), pp. 85–189, and Ralph K. Huitt and Robert L. Peabody, *Congress: Two Decades of Analysis* (New York: Harper & Row, 1969), pp. 3–73.

[3] The recurrent finding of weak or nonexistent constituency influence has been challenged on methodological grounds. See, for example, Morris P. Fiorina, "Constituency Influence: A Generalized Model and Its Implications for Statistical Studies of Roll-Call Behavior," *Political Methodology*, 2 (1975), 249–66.

sideration in legislative chambers than are preferences in other policy areas.[4]

3. Party is more closely associated with legislative voting behavior than any other factor.[5]

Our analysis, as we will show below, lends additional support to these findings.

Despite the growing accumulation of research on the representative process, investigators have given surprisingly little attention to the relationship between representativeness and the simple occurrence of elections. The notion that the necessity of facing reelection will sensitize representatives to the preferences of their constituencies has an honorable tradition in political science. According to this "electoral connection" thesis,[6] even if the general public is often politically ignorant and apathetic,[7] representatives will be responsive to their constituents because ultimately they can be held accountable for their actions. And, as Miller and Stokes have noted, representatives in fact believe that they will be judged by how they vote in the legislative chamber:

> Of our sample of Congressmen who were opposed for reelection in 1958, more than four-fifths said the outcome in their districts had been strongly influenced by the electorate's response to their records and personal standing. Indeed, this belief is clear enough to present a notable contradiction: Congressmen feel that their individual legislative actions may have considerable impact on the electorate, yet some simple facts about the Representative's salience to his constituents imply that this could hardly be true.[8]

Kingdon, furthermore, found that many representatives fear the possibility that a "wrong" vote may be used against them by an opponent in a later election.[9] Finally, recent turnover trends indicate that professional legislative careers are becoming increasingly popular.[10] Legislative careers necessitate reelection.

Voting behavior patterns also suggest that representatives are concerned about their electoral survival. Although the findings have been mixed, studies of the relationship between electoral competitiveness and representativeness suggest a tendency by legislators from marginal districts to be more responsive than those from safe districts.[11] A recent analysis of U.S. Senate roll-call votes indicates a movement toward more moderate positions in election years by both liberal and conservative members.[12] Since both Democratic and Republican elites tend to be more extreme in their respective policy positions than their mass public followers,[13] such shifting suggests a vote-maximizing response by senators to constituency opinion.

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[4] Especially convincing is Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (1963), 45–56.

[5] Although the predominance of party is generally accepted, questions have been raised about its actual influence. Shannon, for example, argues that party position may be determined by the fact that most legislators of a given party face similar demands from constituency majorities. By voting in accordance with these majority preferences, legislators coincidentally establish party position. Thus, party and constituency influences cannot be completely separated in roll-call analysis. W. Wayne Shannon, *Party, Constituency and Congressional Voting* (Baton Rouge: Louisiana State University Press, 1968), Ch. 6. This contention has received empirical support in James H. Kuklinski, "District Competitiveness and Legislative Roll-Call Behavior: A Reassessment of the Marginality Hypothesis," *American Journal of Political Science*, 21 (1977), 627–38.

[6] This thesis most recently has been explicated and articulated in David R. Mayhew, *Congress: The Electoral Connection* (New Haven: Yale University Press, 1974). As Mayhew notes, this response can be manifested in ways other than roll-call voting. One example is the provision of governmental services. See Morris P. Fiorina, "The Case of the Vanishing Marginals: The Bureaucracy Did It," *American Political Science Review*, 71 (1977), 177–81.

[7] There is no consensus on the "true" nature of the typical voter. For a summary of this continuing debate, see the June 1972 issue of the *American Political Science Review*, 415–70; Herbert Asher, *Presidential Elections and American Politics: Voters, Candidates and Campaigns Since 1952* (Homewood, Ill.: Dorsey, 1976), especially Pt. I; and Richard G. Niemi and Herbert F. Weisberg, eds., *Controversies in American Voting Behavior* (San Francisco: W. H. Freeman, 1976), especially Secs. II and III.

[8] Miller and Stokes, "Constituency Influence," 48.

[9] John W. Kingdon, *Congressmen's Voting Decisions* (New York: Harper and Row, 1973), pp. 59–60.

[10] Mayhew, *Electoral Connection*, pp. 13–15. There is some evidence, however, that careerism has begun to decline. See Bruce I. Oppenheimer, "Subcommittee Government and Congressional Reform," *DEA News* (Summer 1976), S8–S11.

[11] The contradictory findings regarding the marginality hypothesis are aptly summarized in John L. Sullivan and Eric M. Uslaner, "Congressional Behavior and Electoral Marginality: Some Considerations Based Upon Spatial Models of Electoral Competition" (paper presented at the Annual Meeting of the Midwest Political Science Association in Chicago, 30 April 1976). The electoral marginality of a district and the simple occurrence of an election are, of course, distinct phenomena. No one has addressed the potentially interesting question of how these two variables interact to affect legislative behavior.

[12] Richard C. Elling, "Patterned Temporal Ideological Instability in the United States Senate: Of Time and Electoral Responsiveness" (unpublished manuscript). See also Dean R. Brimhall and Arthur S. Otis, "Consistency of Voting by our Congressmen," *Journal of Applied Psychology*, 32 (1948), 1–7.

Perhaps the most convincing support for the electoral connection thesis is provided by Dye's analysis of the Pennsylvania house and senate.[14] Relying on rural-urban and income indices, Dye found that members of the lower chamber were strikingly more representative than their senate counterparts. Few have noticed the significance of his concluding conjecture that the longer senatorial terms may moderate the impact of constituency preferences on roll-call voting in that chamber.

There is, then, ample *indirect* evidence that elections function as an effective linkage mechanism between constituencies and representatives. Yet, the most fundamental questions have not been adequately addressed:

1. How does the prospect of an impending election condition the level of policy agreement between representative and constituency?
2. Is the time span between elections an important conditioning influence in the process of representation?

Answers to these questions are central to democratic theory. They also have relevance to those who currently are considering measures which would make our legislative bodies more responsive.

Linkage Studies: Emerging Responses to Past Problems

Buttressed by a growing conceptual and methodological sophistication, political scientists have shown increasing sensitivity to the problems which have consistently plagued past linkage studies. The overriding problem has been a heavy reliance upon surrogate measures of constituency opinion, particularly selected social and economic characteristics of the district. Underlying the use of these measures has been the assumption that opinion within a district is simply an additive linear function of the proportions of the specified demographic characteristics. Challenging this assumption,[15] researchers have begun to search for more direct measures of constituency opinion.

Referendum voting returns have proved a particularly useful measure of constituency opinion.[16] Referendum results appear to be far better than the standard demographic variables for at least two reasons. First, the directness of voting returns as a measure eliminates the need for unwarranted inferential leaps. Second, preferences can be defined in terms of the majority opinions of the *voting constituency.* Scholars recently have attacked the majoritarian model on the grounds that it is naive and simplistic regarding the actual decision-making calculus of a legislator.[17] Despite the numerous disclaimers, however, no one has been able to develop a model which identifies and accounts for a legislator's "real" constituency.[18]

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[13] Herbert McClosky, Paul Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," *American Political Science Review,* 54 (1960), 406–72. This finding has been corroborated in a more recent study of mass and elite opinion: see William Shaffer, Ronald Weber, and Robert Montjoy, "Mass and Political Elite Beliefs about the Policies of the Regime" (paper presented at the Annual Meeting of the American Political Science Association in New Orleans, 7 September 1973).

[14] Thomas R. Dye, "A Comparison of Constituency Influence in the Upper and Lower Chambers of a State Legislature," *Western Political Quarterly,* 14 (1964), 473–80.

[15] Burnham and Sprague provide an excellent general treatment of the additive linear model: Walter Dean Burnham and John Sprague, "Additive and Multiplicative Models of the Voting Universe: The Case of Pennsylvania: 1960–1968," *American Political Science Review,* 64 (1970), 471–90. The additive and linear assumptions are questioned in James H. Kuklinski, "Constituency Opinion: A Test of the Surrogate Model," *Public Opinion Quarterly,* 41 (1977), 34–40.

[16] Wilder W. Crane, Jr., "Do Representatives Represent?" *Journal of Politics,* 22 (1960), 295–99; Ronald D. Hedlund and H. Paul Friesema, "Representatives' Perceptions of Constituency Opinion," *Journal of Politics,* 34 (1972), 730–52; Robert S. Erikson, Norman R. Luttbeg, and William V. Holloway, "Knowing One's District: How Legislators Predict Referendum Voting," *American Journal of Political Science,* 19 (1975), 231–46. An alternative approach to the problem of identifying constituency opinion has been developed as part of Frank Munger's "Two-Party Competition in the American States" project. The technique is described in Ronald E. Weber, *Public Policy Preferences in the States* (Bloomington: Institute of Public Administration, Indiana University, 1972), and Ronald E. Weber, Anne H. Hopkins, Michael L. Mezey, and Frank J. Munger, "Computer Simulation of State Electorates," *Public Opinion Quarterly,* 36 (1972–73), 549–65. This technique is appealing because it is more widely applicable than referendum voting. There is some evidence, however, that public opinion estimates have been confused with the socioeconomic factors that define them. See David Seidman, "Simulation of Public Opinion: A Caveat," *Public Opinion Quarterly,* 38 (1975), 331–42.

[17] The arguments of John E. Jackson have been particularly convincing: see his *Constituencies and Leaders in Congress* (Cambridge: Harvard University Press, 1974), especially pp. 1–12. For more detailed discussions of complex models, see Edwin T. Haefele, "A Utility Theory of Representative Government," *American Economic Review,* 61 (1972), 350–61; Jerome Rothenberg, "A Model of Economic and

Using the majority preferences of the voting constituency moves away from the simple majoritarian model, since it includes only that part of the electorate which is most salient to a legislator—those who have the right to vote and do so. Thus, although constituency is still ultimately defined in a majoritarian sense, it has been more precisely delineated by the exclusion of all but those individuals who show enough political interest to cast their votes. Although this definition of constituency admittedly falls short of some espoused theoretical models, it represents an empirical step in the right direction.

A related problem has been the definition of constituency opinion in a way most useful to scholars attempting to assess the extent of policy agreement between representatives and the represented. "Too often," Key has observed, "discussions of constituency opinion become murky when meticulous scholars try to define their conceptions and form distinctions that will enable them to make statements that seem to fit the observable realities of the interaction of public opinion and government. . . ."[19] Possibly the most evident reality is the lack of information flow from constituents to representatives on specific issues.[20] On most issues, constituency preferences are not communicated to their representatives. Yet, if a legislator is predisposed to follow his constituents' opinion when voting on a vill, he obviously is concerned with constituency sentiment on that specific issue.

In their efforts to resolve this apparent dilemma, students of legislative behavior increasingly have emphasized the importance of policy concepts or general evaluative dimensions to the representative process. These policy concepts are no more than general categories into which specific political issues are classified. Their importance derives from their function as a communication mechanism.

> It is through the use of policy concepts of high generality that political discourse is possible. A policy concept provides a summary of the detailed statement of a policy proposal in shorthand terms understood by others. . . . We [should] rid ourselves of the illusion that people, like ourselves, do more than assign policies into broad categories for evaluative purposes. . . .[21]

When representatives make a determination on a bill, the role of more general evaluative dimensions should be important. For if representatives know the general disposition of their constituencies, they can use this understanding to guide their specific legislative acts, thus at least partially compensating for the lack of continuous constituency communication. The level of representative-constituency policy agreement should be more clearly specified in terms of overall tendencies than specific issues.

Construction of Policy Dimensions

Taking cues from these recent efforts, this paper looks at the linkage between representatives and constituencies in terms of policy dimensions. Our data base at the constituency level consists of votes by the California electorate on state-related referenda and initiative issues coming before them during the 1968, 1970, and 1972 general elections; a total of 33 propositions were included.[22] An inter-item correlation matrix, based on Pearson's r, for district votes on each of the 33 propositions was computed and factor-analyzed.[23] Although we initially extracted and rotated five dimensions, we retained only the first three for use in subsequent analysis.[24] These dimensions, and the variables loading significantly on them, are presented in Table 1.

Political Decision-Making," in Julius Margolis, ed., *The Public Economy of Urban Communities* (Washington, D.C.: Resources for the Future, 1965), pp. 1–38; and John A. Ferejohn, *Pork Barrel Politics: Rivers and Harbors Legislation, 1947–1968* (Stanford: Stanford University Press, 1974).

[18] Clausen, for example, argues that a representative's "real" constituency consists of those who voted for him in the last election. Yet, his analysis is ultimately based on the legally specified constituency. Aage R. Clausen, *How Congressmen Decide: A Policy Focus* (New York: St. Martin's, 1973), Ch. 6.

[19] V. O. Key, *Public Opinion and American Democracy* (New York: Knopf, 1967), p. 8.

[20] Ibid., Ch. 16.

[21] Clausen, *How Congressmen Decide*, pp. 16–17.

[22] There were a total of 51 propositions for the three elections. Unfortunately, incomplete county returns for 18 propositions precluded their use in this study.

[23] The factor analysis was done with unities as the estimates of communalities and an orthogonal (varimax) rotation was used to obtain the matrix of factor loadings. The orthogonal rotation was selected as optimal.

[24] Two dimensions were discarded because of the lack of high loadings.

The constituency policy dimensions derived by the above process are similar for assembly and senate districts. In fact, we have determined the character of the three pairs of equivalent dimensions by the loadings of the same variables. The identification of similar dimensions among both types of constituencies adds credence to the findings, even though assembly and senate district lines tend to parallel each other.

The first general category of issues comes under the heading of *contemporary liberalism.* Among the issues characterizing this policy domain are civil rights, labor union support and a set of "social issues," including legalization of marijuana, definition of obscenity, opposition to capital punishment, improvement of the environment, and extension of the vote to 18-year-olds. The latter issues are very representative of those which gained prominence during the late 1960s, and, generally speaking, "apply to all conflict between the forces of change and the forces of resistance to deviations from the traditional moral values."[25]

The second constituency policy dimension is concerned with issues of *taxation.* As one writer has noted, "The processes of urbanization and industrialization inevitably translate themselves into increased public costs that must be financed if orderly economic growth and life are to be realized."[26] Given the rapid growth of California's population and its expanding economy, substantial expenditure responsibilities have been placed on the state and its local units of government. As a result, extensive attention has been given to developing an adequate system of state-local taxation.

Those variables defining the third and final dimension relate to questions of *government administration.* It is important to note that these questions are concerned only with changes of procedure and administration within the state governmental process. Questions such as government management of the economy and the financial resources of the state are not included.[27]

The identification of these three dimensions provides at least minimal empirical evidence that there are widely shared policy concepts which have strong footholds among the mass public. They also form the basis upon which legislative dimensions were constructed.

We initially aggregated by session and chamber all bills which might conceivably lie on one of the three dimensions—contemporary liberalism, taxation, or government administration. Each set of roll calls then was subjected to a variant of factor analysis first suggested by Charles Spearman.[28] This procedure eliminated all roll calls which did not fall onto the assumed dimension.[29]

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[25] Robert S. Erikson and Norman R. Luttbeg, *American Public Opinion: Its Origins, Content, and Impact* (New York: Wiley, 1973), p. 54.

[26] Gerhard N. Rostvold, *Financing California Government* (Belmont, Calif.: Dickenson Press, 1967), p. xi.

[27] This point is made to distinguish government administration and government management. For a full discussion of the latter, see Clausen, *How Congressmen Decide.*

[28] Charles Spearman, "General Intelligence, Objectively Determined and Measured," *American Journal of Psychology,* 15 (1904), 201–93. We used Spearman's two-factor solution, as opposed to more common techniques, for several reasons. First, the two-factor solution is particularly appropriate when the existence of a single factor is a viable hypothesis. Since our initial lists of bills were determined by the previously identified constituency policy dimensions, the existence of a single factor was assumed. Second, if Guttman scaling, the usual scaling technique in political science, had been used, a large number of bills falling on the various dimensions would have been excluded from subsequent analysis. Our objective was to ensure unidimensionality yet retain as many bills as possible; Spearman's technique met this objective. Finally, most of our attention in the following pages will be focused on the contemporary liberalism dimension. As Weisberg has noted, the Guttman scale may not be appropriate when the direction of social change is at issue. See Herbert F. Weisberg, "Scaling Models for Legislative Roll-Call Analysis," *American Political Science Review,* 66 (1972), 1306–15. It is not our intention to discuss the relative merits of the order-relations and proximity-relations models. Such questions are adequately raised and handled in the Weisberg article.

Particularly helpful references to the technical and theoretical applications of Spearman's test of unidimensionality include: Sir Godfrey Thomson, *The Factorial Analysis of Human Ability* (Boston: Houghton-Mifflin, 1939); B. Fruchter, *Introduction to Factor Analysis* (New York: Van Rostand, 1954); and Harry H. Harman, *Modern Factor Analysis,* 3rd ed. (Chicago: University of Chicago Press, 1976).

The problem of absentee voting was also given considerable attention. Our ultimate decision was to exclude absences in the scoring. The basic assumption underlying this decision is that the roll-call votes on which no absences are observed, with respect to each legislator, are a representative sample of the set of roll calls in the scale. A careful review of the absences gave us no reason to believe that the remaining roll calls were unrepresentative of the whole set for any legislator. In addition, legislators having an absentee rate of 50 percent or more on a particular dimension were excluded from further analyses. Our final Ns were: 1970 senate = 39; 1971–1973 senate = 40; 1970 assembly = 73; 1971 and 1973 assembly = 75; 1972 assembly = 76.

[29] The analysis is based on a tetrachoric r matrix. The applicability of tetrachoric r to dimensional analysis is discussed in Duncan MacRae, Jr., *Issues and*

Table 1. Constituency Policy Dimensions for Assembly and Senate Districts, Varimax Solution

	Contemporary Liberalism	Taxation	Government Administration
Removes state penalties for personal use of marijuana.	.956 (.954)	–	–
Amends, deletes, and adds penal code statutes relating to obscenity; deletes "redeeming social importance test."	–.865 (–.849)	–	–
Authorizes use of revenues from motor vehicle fuel tax and license fees for control of pollution caused by motor vehicles.	.853 (.908)	–	–
Creates State Coastal Zone Conservation Commission and six regional commissions. Establishes permit area and prohibits development within area without permit.	.821 (.830)	–	–
Reimposes death penalty; death penalty shall not be deemed to be cruel or unusual punishment.	–.800 (–.848)	–	–
Sets forth permissible and prohibited labor relation activities of agricultural workers, employers and labor organizations; defines unfair labor practices.	–.788 (–.778)	–	–
Requires provision for primary elections for partisan offices, including open presidential primary; gives 18-year-olds the right to vote.	.725 (.623)	–	–
Adds section to education code providing: "No public school student shall, because of his race, creed or color be assigned to or be required to attend a particular school."	–.689 (–.532)	–	–
Permits approval by majority, rather than two-thirds, vote to pass bond issue for repairing or replacing unsafe public school buildings.	.657 (.578)	–	–
Provides for a bond issue of $250,000,000 for water pollution control funds, including grants to local governments.	.610 (.643)*	–	–
Authorizes legislative issuance of revenue bonds to finance installation of environmental pollution control facilities.	.585 (.686)	–	–
Eliminates property tax for welfare purposes; limits property tax for education.	–.571 (–.527)*	–	–
Adds right to privacy to inalienable rights of people.	.485 (.405)	–	–
Requires meetings of U of C regents to be public.	–	–	.972 (.966)
Provides speaker of assembly shall be ex officio member of any agency charged with administration of state college system.	–	–	.964 (.936)
Continues existing civil service system, with revisions.	–	–	.962 (.943)
Provides Supreme Court has exclusive jurisdiction to determine questions of vacancy in executive offices except governor's.	–	–	.954 (.928)
Requires governor to submit budget to Legislature within first ten days of each regular session.	–	–	.953 (.936)
Requires that county governing body shall prescribe compensation of its members by an ordinance subject to referendum.	–	–	.949 (.938)

Particularly important to this study is the need to ensure equivalency between the constituency and legislative policy dimensions. To provide at least minimal evidence that they are equivalent, we determined whether each legislative dimension contained at least some bills having substance similar to the propositions loading on the relevant constituency dimension.

Defining the term "similar" is difficult because, understandably, legislative bills and ballot propositions are not exactly worded. To illustrate the meaning of similarity as used in this paper, consider three bills introduced in the 1971 assembly which were originally included in the category of contemporary liberalism:

AB 624–Prohibits imposing additional penalties on the basis of prior convictions for felony narcotics and restricted dangerous drug convictions if defendant was addicted to such drugs at the time of his present offense and if quantity of drugs in his possession was sufficient for his own use.

AB 627–Includes persons convicted of cultivating or harvesting marijuana under requirement of registration with city police. Excludes from requirement persons convicted of a misdemeanor for possession of marijuana.

Table 1. (continued)

	Contemporary Liberalism	Taxation	Government Administration
Authorizes one additional deputy superintendent of public instruction exempt from civil service.	–	–	.927 (.912)
Provides minimum tax exemption for owner-occupied dwellings; provides for grants to local government for lost revenue.	–	.969 (.966)	–
Provides that lands located outside of the county, city and county or municipal corporation (including any public district or agency) owning the same, which were taxable when acquired, shall be assessed in accordance with prescribed formula based on total population and assessed value in the state.	–	–.931 (–.936)	–
Deletes provision requiring two-thirds vote by Legislature to limit amount of taxes a county may impose on property; replaces existing provisions fixing minimum amount of money to be provided annually for support of public schools with requirement that Legislature grant basic state financial aid to each school district.	–	.880 (.895)	–
Provides for a bond issue of $250,000,000 for water pollution control funds, including grants to local governments.	–	.638 (.630)*	–
Eliminates property tax for welfare purposes; limits property tax for education.	–	–.555 (–.551)*	–

Note: Variables with loadings of less than .400 are omitted from this table, but did enter into the computation of district scores on each factor.

*Indicates variable loaded on more than one factor for both types of districts. Parentheses indicate loadings by senate districts.

Parties in Legislative Voting (New York: Harper & Row, 1970), Ch. 3, and Herbert F. Weisberg, "Dimensional Analysis of Legislative Roll Calls" (Ph.D. dissertation, University of Michigan, 1968). In the strictest sense, factor analysis is based on a Pearson correlation matrix. The major consequence of relaxing this assumption was the elimination of a few bills on each original list which otherwise might have been retained. For more general and lucid discussions of considerations involved in selecting appropriate measures of statistical relationship, see MacRae, *Issues and Parties*, esp. pp. 98–110, and Herbert F. Weisberg, "Models of Statistical Relationship," *American Political Science Review*, 68 (1975), 1638–55. Their discussions of the sensitivity of the various models to the marginals is particularly helpful.

AB 1143–Permits school district to employ person convicted of use or possession of marijuana if it determines he has been rehabilitated for at least five years.

Although all three bills obviously deal with the general subject of drugs, only the latter two are defined as similar in content, since the pertinent proposition on the contemporary liberalism dimension deals only with the legalization of marijuana, and not other drugs. In short, a bill is defined as similar only when it refers directly to the specific subject matter of a proposition.

On this basis, we determined that every legislative dimension includes at least several bills similar in content to the propositions defining the relevant constituency policy dimension. This similarity, combined with the use of Spearman's technique to measure unidimensionality, provides reasonable assurance that the constituency and legislative dimensions are equivalent.

Policy Representativeness: A Preliminary Analysis

We have proposed the use of policy dimensions to measure the extent of agreement between legislative roll-call behavior and constituency opinion.[30] As Table 2 indicates, our data strongly support the patterns identified by earlier research. *Overall,* the level of policy agreement between legislative voting and constituency opinion is low for both houses throughout the entire four-year period. To the extent it is meaningful to talk in terms of "average" representativeness, the mean simple correlation between roll-call votes and district opinion in the assembly is .22; the equivalent relationship within the senate is an even more meager .14. When party is controlled, the relationships do not change significantly, indicating an independent effect of constituency.

Perhaps more revealing is our finding that the level of representation varies across the three policy dimensions. As expected, in both chambers representativeness is substantially higher on contemporary liberalism than the other two dimensions. Once again, the relative importance of constituency opinion on questions of individual rights is clearly indicated.

It might be hypothesized that the higher level of representation on contemporary liberalism issues is due primarily to their greater importance to the electorate. The media, for

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[30] Accordingly, our measure of policy agreement is the correlation of the appropriate factor scores. It is these correlations which are reported throughout the remainder of this paper.

Table 2. Simple and Partial[a] Correlations of Constituency Opinion, Party, and Roll-Call Behavior, Assembly and Senate, 1970–73

	Assembly		Senate	
Roll-Call Behavior	**Constituency Opinion**	**Party**	**Constituency Opinion**	**Party**
Contemporary Liberalism				
1970	.43* (.42)	.83* (.83)	.30** (.30)	.67* (.75)
1971	.44* (.44)	.86* (.85)	.15 (.28)	.83* (.84)
1972	.43* (.42)	.89* (.88)	.28 (.41)	.80* (.83)
1973	.38* (.37)	.83* (.80)	.19 (.19)	.77* (.78)
Taxation				
1970	.09 (.08)	.74* (.75)	.16 (.14)	.51* (.56)
1971	.05 (.05)	.89* (.89)	.13 (.13)	.70* (.72)
1972	.06 (.16)	.81* (.81)	.02 (.22)	.80* (.81)
1973	.04 (.04)	.85* (.84)	.03 (.09)	.56* (.56)
Government Administration				
1970	.18 (.09)	.70* (.76)	.13 (.13)	.82* (.81)
1971	.16 (.14)	.91* (.92)	.09 (–.03)	.83* (.83)
1972	.12 (.02)	.80* (.78)	.18 (.15)	.76* (.76)
1973	.13 (.12)	.42* (.42)	.06 (.06)	.72* (.71)

[a]Partial correlations are in parentheses.
*Simple correlation significant at .01 level.
**Simple correlation significant at .05 level.

example, will be inclined to dramatize issues such as reinstatement of the death penalty and legalization of marijuana.[31] To test this proposition, a saliency index was constructed:

$$\text{Index of Saliency} = \frac{\frac{P_1}{T} + \frac{P_2}{T} + \frac{P_3}{T} + \ldots . \frac{P_N}{T}}{N}$$

where,

P_1, P_2, P_3 . . . P_N = total district votes on those propositions loading .400 or more on the dimension (see Table 1);

T = total district vote for president (1968 and 1972 election) or governor (1970 election);

N = total number of propositions with loadings of .400 or more on the dimension.

Using this index, it was found that, for every assembly and senate district, contemporary liberalism in fact is the most salient dimension. More significant, however, with few exceptions the taxation dimension is nearly as salient. The average saliency score on contemporary liberalism is .94, as compared to .91 and .78 on taxation and government administration, respectively. Saliency itself does not sufficiently explain the differential in representativeness on the contemporary liberalism and taxation dimensions.

One factor which might help to explain this apparent discrepancy is the nature of the dimensions themselves. Whereas issues comprising the contemporary liberalism dimension often are viewed in normative terms, taxation legislation tends to be rather complex and technical in nature. Consequently, as we shall show in later research, legislators consider themselves more knowledgeable about taxation policies vis-à-vis their constituents than other policy areas.[32] In part, this may be due to the legal training of many of the legislators, since taxation questions comprise a significant part of a legal education.

Finally, the strong relationship between legislative voting and party should be noted. In only three instances is the partial correlation between party and legislative voting (controlling for constituency opinion) less than .75. This finding lends support to the many studies which acknowledge the political party of a representative as the single most important factor in legislative voting.

Elections as a Conditioning Influence

As previous studies have also demonstrated, we have found low to moderate levels of policy agreement between representatives' roll-call behavior and constituency preferences. We now shall employ the representational model suggested by MacRae[33] to examine how this general level of policy agreement is modified by the temporal proximity of an election.

According to this model, the greater the effect of a roll-call vote on the probability of reelection, the more likely the legislators are to vote in accordance with constituency preferences. If one accepts the proposition that votes cast closer to legislators' reelection campaigns have more impact on their chances for reelection, then, according to MacRae, legislators will exhibit greater concern for constituency preferences closer to an election.

MacRae's basic premise is tested in Figure 1 for the contemporary liberalism dimension.[34] The model is particularly suitable to our data, since only 20 of the 40 California senators run in a given election. Accordingly, senators have been divided into two classes: those who ran in 1970 and those who ran in 1968 and/or 1972. The accuracy with which the model posited by MacRae predicts senatorial voting is striking. For those senators running in 1970, policy agreement between roll-call voting and constituency opinion is .41 for that year. Following the election, the level of representation drops precipitously and continuously, reaching a low of .07 in 1973. Looking at those senators running in 1972, we find a low level of representation for the years 1970 and 1971. In the election year 1972 the correlation between roll-call voting and constituency opinion jumps to .57.[35] Following the election, representativeness again drops to a level consistent with that of the nonelection years 1970 and 1971.

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[31] These two issues received widespread coverage during the 1972 campaign.

[32] Interviews were conducted with assemblymen during August 1974.

[33] Duncan MacRae, Jr., *Dimensions of Congressional Voting* (Berkeley: University of California Press, 1958), Appendix C. For an economic argument which leads to the same conclusion reached by MacRae, see Albert Breton, *The Economic Theory of Representation* (Chicago: Aldine, 1974).

[34] We have chosen the contemporary liberalism dimension because of the higher level of representation on it and also because of the lack of any discernible pattern on the other two dimensions.

[35] This correlation is particularly impressive when it is remembered that both sets of data, constituency and legislative, were previously factor analyzed, thus removing some of the original variance in our data.

Figure 1 also indicates that, in contrast to senatorial voting, the roll-call behavior of assemblymen does not fluctuate in response to an oncoming election. The overall level of representation for that body is not any greater during an election year than during an off-year. Furthermore, the extent of policy agreement remains at a relatively high level among assemblymen, particularly when compared to the extent of representation by senators during the nonelection years.

Because our study covers only four years, any conclusions regarding the above patterns must be viewed as tentative. Nonetheless, Figure 1 does suggest that the time span between elections is integrally related to the overall level of representation.[36] Once elected, senators apparently feel free to disregard constituency preferences until the next reelection year—some three years hence. In contrast, assemblymen must come before their constituents every two years. As a result, they essentially are subjected to a continuing electoral process. A statement by a former governor in support of a four-year term of office summarizes very nicely the situation in which California assemblymen find themselves:

> Having to face reelection every two years, [officials] are forced to campaign . . . practically full time. Win one election and begin campaigning for the next. To do otherwise would be political suicide.[37]

Confronted by closely spaced elections, then, assemblymen must be constantly attuned to the opinions and preferences of their constituents.[38] Not only is this fact instrumental in explaining the continuing relatively high level of representation by assembly members, but it also supports our Founding Fathers' belief that the length of legislative terms would condition the representational linkage. Samuel Adams expressed his concern in strong rhetorical terms: "where annual elections end, tyranny

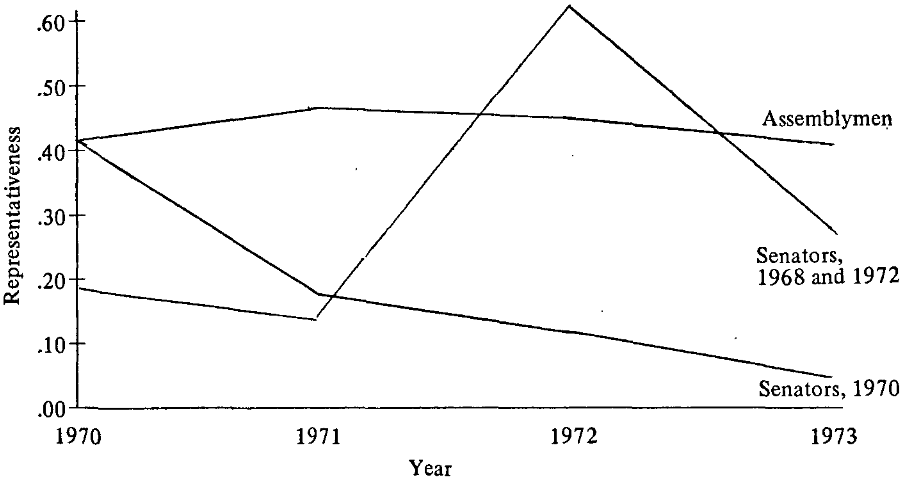

Figure 1. Relationship Between the Temporal Proximity of Elections and Representativeness

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It should be noted that the differences reported in this section are not an artifact of different variances of the variables resulting from partition of the data.

[36] Figure 1 also suggests that representatives *do* respond. Eulau and Karps argue recently that researchers have not provided evidence of response, as opposed to simple congruence. See Heinz Eulau and Paul K. Karps, "The Puzzle of Representation: Specifying Components of Responsiveness," *Legislative Studies Quarterly*, 2 (1977), 233–54. Stone also has used a longitudinal analysis to provide evidence of response: Walter J. Stone, "A Panel Analysis of Representation in Congress: A Preliminary Report" (paper presented at the annual meeting of the American Political Science Association, Washington, D.C., 4 September 1977).

[37] This statement was made by former Iowa Governor Harold Hughes, *Cedar Rapids Gazette*, 20 April 1964.

[38] It might be argued that the greater overall representativeness by assemblymen can be attributed to district size. The high levels of representation by senators during election years indicate that the difference cannot be explained by this factor.

begins."[39] Other supporters of short terms of office also voiced their opinions in a more measured way when they argued that "frequent elections are unquestionably the only policy by which government dependence on, and sympathy with, the people can be effectually secured"[40]—a contention which seems quite prophetic in light of the data.

Citing the Federalists, we are reminded that the American states operate not as independent and autonomous entities, but as units of the nation.[41] Consequently, examination of the relationship between elections and representativeness in a state setting ought to consider the possible conditioning influence of the larger context.

The national event likely to have the most consistent impact on the roll-call behavior of state legislators vis-à-vis their constituents is the presidential election.[42] Characterized by highly visible campaigns, made so in large part by extensive media coverage, the presidential election works effectively to highlight issues as well as party differences on them. Because the state legislator has no control over the national issues which ultimately evolve in an election, his uncertainty and anxiety concerning its outcome necessarily increase. Key has noted the importance of the outcome of a presidential election to state office holders:

> The presidential candidate who leads his party to a landslide victory carries into office with him large numbers of state candidates of his party, who win without much regard to their role or place in the state but because they float along with the national movement of sentiment. Similarly, those state candidates allied with the losing party on the national scene are caught up by the common misfortunes of their partisan allies over the nation.[43]

Extending the logic of this argument, one would expect representativeness to be highest in presidential election years, since state legislators no doubt will strive to compensate for any national factors which might jeopardize their chances of success.

Looking once again at Figure 1, it is clear that those senators running in 1970 are less representative of their constituencies' preferences during that election year than those running in 1972, a presidential election year. For the former, the correlation between roll-call voting and constituency opinion is .41, while for the latter it is .57—a substantial difference of .16. Additionally, the drop in representativeness following the two elections is much greater among those running in 1972. The level of representation among the senators running in the nonpresidential election drops from .41 to .24, a noteworthy decline. For those senators who ran in 1972, however, the declivity is nearly *twice* as great, a further indication of the importance of presidential elections to California senator-constituency relationships.

The importance of presidential campaign issues to the level of representation is emphasized even further when the correlations *within* party are considered. For Democratic senators running in 1972, who undoubtedly felt greater uncertainty given the prediction of a Nixon landslide, the correlation of constituency opinion and roll-call behavior is .68. For Republicans running in 1972, the correlation is .50, substantially less, but certainly still high. Democratic senators, then, had greater reason than Republican senators to buffer themselves from the 1972 presidential election, and did so by voting more closely in line with their constituencies' wishes. Yet, the 1972 class of Republicans, even though likely to ride the coattails of Nixon's expected overwhelming victory, also apparently paid greater attention to constituency preferences than did their 1970 counterparts. The equivalent correlation for those Republican senators running in 1970 is .39, .11 less than those incumbent Republicans facing reelection in 1972.

But why don't assemblymen show the same differential response to presidential and nonpresidential elections as state senators? Figure 1 very clearly shows that representativeness among members of the lower chamber is *not* greater in 1972 than 1970. If our argument about the impact of presidential campaigns on senatorial response is to be credible, the apparently anomalous behavior of assemblymen must be explained. This behavior, we suggest, has already been explained in terms of the continuous electoral process to which assemblymen are subjected. Having to face reelection every two years, members of the lower chamber feel they must maintain an acute awareness of constituency preferences from session to session. Apparently, most assemblymen believe that the level of representation shown in Table 1 will bring success in the next election, whether a presidential one or not.[44] The fact that incumbent Democratic assemblymen, unlike their senate counterparts, are not more representative than Republican members of the lower chamber during the 1972 session supports this contention.[45]

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[39] Cited in *The Federalist Papers*, No. 53.

[40] *The Federalist Papers*, No. 52. James Madison, the author of both numbers 52 and 53, himself favored a longer term of office.

[41] This theme is echoed throughout V. O. Key, *American State Politics: An Introduction* (New York: Knopf, 1966).

[42] Malcolm E. Jewell and Samuel C. Patterson provide one of the better summaries of the impact of presidential elections on constituency voting for state legislators. See their *The Legislative Process in the United States*, 2nd ed. (New York: Random House, 1973), pp. 124–31.

[43] Key, *American State Politics*, p. 32.

The relationship of roll-call voting and party position provides final support for the general thrust of our findings. As we noted earlier in Table 2, this relationship is quite high and constant across the four years of our study for both assemblymen and senators *as a whole.* When senators are divided into two classes, however, it becomes evident that they tend to be more disloyal to their party during election years (Table 3). For senators running in 1970, the partial correlation of roll-call voting and party is lowest in that year; similarly, it is lowest in 1972 for the class facing reelection in 1972. Given our incomplete knowledge about the determinants of party position, any interpretation of these findings is tentative and exploratory.[46] It would appear, nonetheless, that assemblymen have created a complex system whereby party position serves to further the reelection interests and needs of most members. However, senators, elected from more heterogeneous districts and typically displaying less concern for constituency opinions and preferences, have not developed this intricate system to the same extent. Consequently, during election years, they find that the requisites of reelection necessitate a greater degree of party disloyalty.

Table 3. Partial Correlations[a] of Party and Roll Call Behavior, by Senate Class, Contemporary Liberalism Dimension, 1970–73

	Senate Class	
Year	1968/1972	1970
1970	.82	.71
1971	.83	.85
1972	.67	.90
1973	.82	.85

[a] *The controlled variable is constituency opinion.*

Concluding Comments

Looking at the linkage between constituency preferences and a representative's roll-call behavior in terms of broad evaluative dimensions, we have found that the overall extent to which district opinion influences roll-call voting varies across three policy domains. On taxation and government administration, we found little relationship between constituency preferences and representatives' roll-call behavior. Only on contemporary liberalism was district opinion an important determinant of roll-call voting.

Utilizing a comparative and modified longitudinal analysis, we also found that the level of policy agreement is conditioned by the need of incumbents to win reelection. Particularly important is the frequency with which this task must be faced. Assemblymen, who are elected for two-year terms and thus face a continuing electoral process, exhibited a relatively strong sensitivity to constituency opinion in both election and nonelection years. Because they already were attuned to the wishes of their districts, assemblymen did not display a greater sensitivity to constituency opinion when uncontrollable events such as presidential campaigns took place. In contrast, senators, who are elected for four-year terms, exhibited a low level of representation in nonelection years, a level which increased dramatically in election years. Senators, moreover, tended to compensate for any possible negative impact a presidential campaign might have on their own reelection success. Thus, senators running for reelection in presidential years were more representative than their counterparts who faced reelection in nonpresidential years.

Emphasizing even further the impact of the frequency of elections on representativeness is the fact that assemblymen apparently have developed a complex system whereby party position generally reflects district preferences, at least for most members. Senators have not developed this system to the same extent, in part because they tend to ignore district preferences in nonelection years. They they do face a reelection year, therefore, they find it necessary to frequently bolt party lines.

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[44] The findings also may say something about the *limits* of representativeness, at least as political scientists have measured it.

[45] In fact, they were slightly *less* representative. Correlations for Democrats and Republicans were .41 and .44, respectively.

[46] See footnote 4 for a discussion of the basic problem. An adequate test for constituency versus party influence would be to observe the relationship between party-roll-call voting correlations and constituency-roll-call voting correlations across the four years in each chamber. However, given the high correlations of party and roll-call voting, including the lack of variance thereof, we do not have the requisite data base.

The finding that representatives are more loyal to the wishes and preferences of the represented when they face frequent elections brings us, full circle, back to the normative question mentioned at the beginning of this paper: *should* the preferences of the represented be an important determinant of the behavior of the represented? Our Founding Fathers could not reach a consensus on this question 200 years ago, so they compromised; this compromise has affected greatly the character of nearly all legislative bodies in the United States. Today, supporters of the notion that representatives should be no more than delegates of those who sent them will take heart in the finding that frequent elections work to ensure representativeness by members of our lower chambers. Similarly, those who believe that representatives should pursue what they themselves believe to be in the best interest of the mass public may find consolation in the fact that members of most upper chambers face less frequent elections. The great compromise is as alive today as it was 200 years ago.

A Test of Downsian Voter Rationality: 1964 Presidential Voting*

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Systematic testing of Downsian voter rationality is accomplished using a computer simulation technique on the 1964 SRC voting survey. The simulation tests both the hypotheses predicting whether an individual will vote and for whom an individual will vote. To evaluate the results of the tests we develop a statistic analogous to Pearson's r. This statistic measures the percentage improvement over a random guess technique. Utilizing this statistic, Downs explains 68.5 percent of the unexplained variance in the voters' choices of party. Three alternative interpretations of the turnout decision are then considered, each premised on a different notion of how the costs of voting are distributed among the voters. Here we use an Engel Curve technique to develop the turnout decision and explain 92 percent of the variance. The importance of the various elements of the Downsian theory are evaluated and, in contrast to some recent conjectures, the probability of making a difference on the outcome of the election is shown to have an effect on the turnout decision. Finally, to determine the viability of the results, the SRC "6 factor" model is developed in an analogous fashion and used to predict both turnout and direction of vote.

Operationalizing and testing theories are crucial steps in science. Until recently, a dearth of formal models in political science made it difficult for students of politics to concentrate on these activities. Even now after a number of formal theoretical arguments about politics have been developed,[1] systematic tests of these formal models have been slow to emerge.[2]

Voting was among the earliest subjects to be examined by formal political theorists,[3] and the analysis of voting has remained a focal point of theoretical research in the field of political science. One of the first serious attempts to formalize a theory of voting is contained in Anthony Downs' well-known work, *An Economic Theory of Democracy.* Given the central place of Downs' work in this school of thought, it is surprising that little effort has been made to subject this theory to systematic empirical testing.

This does not mean that there has been no attempt to test the Downsian model. A number of scholars have tested specific aspects of his work, especially those propositions relating to spatial competition.[4] Other tests of Downs' work focus on the salience of issues in the calculus of voting.[5] Most recently, Shaffer has

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[1] For a survey and introduction to these theories, see William Riker and Peter Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice Hall, 1974), or Norman Frohlich and Joe A. Oppenheimer, *Modern Political Economy* (Englewood Cliffs, N.J.: Prentice Hall, 1978).

[2] Some efforts have been made to test these theories. Many of these tests are referenced in the footnotes of Frohlich and Oppenheimer, *Modern Political Economy,* and contained in the bibliography of Riker and Ordeshook, *An Introduction.*

[3] A review of most of these is contained in Frohlich and Oppenheimer, *Modern Political Economy,* Ch. 5.

[4] A comprehensive summary of these hypotheses is contained in Riker and Ordeshook, *An Introduction.* Also see Peter Ordeshook, "The Spatial Theory of Elections: A Review and a Critique," in *Party Identification and Beyond,* Ian Rudge, et al., ed. (London: Wiley, 1976), pp. 285–314.

[5] For relevant examples consult David E. RePass, "Issue Salience and Party Choice," *American Political*

conducted a pioneering test of the central aspects of the Downsian argument.[6] Using the 1964 presidential election data assembled by the Survey Research Center, Shaffer focuses on Downs' ability to predict the behavior of the individual voter.

This essay deals with the same basic issues and utilizes the same data base. Specifically, we ask how well Downs' theory predicts individual voting or abstention and individual party preference.

The Downsian Voter

The Downsian model of voting rests squarely on the assumption that individual voters are rational: they evaluate available alternatives in terms of their benefits and costs. Specifically, Downs postulates that voters maximize their expected "utility" in making these calculations. That is, each voter discounts possible benefits and costs by (subjective) probabilities associated with their receipt. These discounts are multiplicative.

The alternatives available to each voter, as a voter, are apt to be highly restricted. To begin with, voters can choose to vote or to abstain. If they choose to cast a vote, they can choose which party, candidate, or side to support. Downs argues that rational voters will restrict their calculations to those potential benefits and costs of voting which are explicitly political. Thus:

> The political function of elections in a democracy, we assume, is to select a government. Therefore, rational behavior in connection with elections is behavior oriented toward this end and no other. Let us assume a certain man prefers party A for political reasons, but his wife has a tantrum whenever he fails to vote for party B. It is perfectly rational *personally* for this man to vote for party B if preventing his wife's tantrums is more important to him than having A win instead of B. Nevertheless, in our model such behavior is considered irrational because it employs a political device for a nonpolitical purpose.[7]

Focusing on those benefits and costs which are specifically political, voters consider "streams of utility derived from government activity."[8] But not all government programs are relevant to the voting decision. The relevant benefits and costs for individual Downsian voters are those which arise from the differences perceived among the programs (or actions) offered by the parties (or candidates) in a given election. Specifically, voters must make future-oriented judgments concerning the utility income they would be likely to receive from each of the parties or candidates for whom they are permitted to vote. The resultant calculations are relatively straightforward when there are only two candidates for a single office. (In fact, this is the only case for which Downs develops his theory fully.)[9] Voters compare the programs they expect one side to carry out over the next election period with the programs they associate with the opposition. Thus:

> Each citizen in our model votes for the party he believes will provide him with a higher utility income than any other party during the coming election period. . . . In a two-party system, this comparison can be set up as a simple subtraction:
>
> $$E(U^A{}_{t+1}) - E(U^B{}_{t+1}).$$
>
> The difference between these two expected utility incomes is the citizen's *expected party differential.*[10]

But this procedure for comparing parties or candidates exhibits three inherent difficulties. First, the voter must employ some "rules of thumb" in order to project what candidates or parties are likely to do once they are in office. Downs suggests various techniques to deal with this problem, including both the utilization of current performance as an indicator or probable future behavior and the reliance upon trends.[11]

Second, the expected party differential alone is not a sufficient basis for reaching reasonable decisions concerning whether to cast a vote or to abstain. After all, individual voters are aware that their vote is unlikely to make a difference in the outcome of the election.

Science Review, 65 (June 1971), 389–400, and John E. Jackson, "The Importance of Issues and Issue Importance in Presidential Elections: A Test of a 'Rational' Model," paper prepared for the annual meeting of the Public Choice Society, Pittsburgh, Pennsylvania, May 1972.

[6] William R. Shaffer, *Computer Simulations of Voting Behavior* (London: Oxford University Press, 1972).

[7] Downs, p. 7.

[8] Downs, p. 36.

[9] Ibid., Ch. 9 contains a brief sketch of the kinds of changes that would be required to extend the analysis to multisided races.

[10] Ibid., p. 39. In this formulation, $t + 1$ is the period of incumbency of the government to be elected, $E(U)$ symbolizes the expected value of U, and U^A and U^B are the utility streams flowing from the alternative governments.

[11] Ibid., pp. 40–45.

Accordingly, the voter "must discount his party differential greatly before arriving at the value of voting correctly. This *vote value* is compounded from his estimates of his party differential and of the probability that his vote will be decisive."[12] Thus, the value to any individual of voting (as opposed to abstaining) is extremely small, and it varies with the likelihood of the individual's vote making a difference.

Third, it is usually irrational to make informed voting decisions. For the costs of becoming informed are apt to be far greater than the benefits to be gained from "informed" voting. This is because the expected benefits from voting for the "best" party or candidate are discounted by the probability of one's vote making a difference. Since these benefits are small there is only a small incentive to invest in information. Therefore, rational voters will avoid the costs of being well informed and look for cost-cutting ways of gathering information. For example:

> some rational men habitually vote for the same party in every election. In several preceding elections, they carefully informed themselves about all the competing parties, and all the issues of the moment; yet they always came to the same decision about how to vote. Therefore they have resolved to repeat this decision automatically without becoming well-informed, unless some catastrophe makes them realize it no longer expresses their best interests.[13]

Alternatively, individuals can avoid the costs of becoming fully informed by voting on the basis of some sort of "ideology":

> The man who uses his ideology differential as a costsaver knows something about current affairs. But he does not know as much as a citizen using issues to make his decisions, because there are many more issues than philosophic axioms in politics.[14]

In summary, Downs' party differential (discounted by the probability of making a difference) is composed of observed differences between the parties on issues together with various surrogates for these issue differences. The resultant difference constitutes the total benefit of voting when voting is viewed as an attempt to determine the outcome of the election at hand.

Bur Downs introduces a second function of voting. Specifically, he hypothesizes that an individual's benefits from the government are partially associated with the electoral process per se. Thus voting yields a benefit to the extent that it supports the democratic process: "Hence democracy is in one sense a reward for voting. We call the part of this reward the citizen receives at each election his *long-run participation value.*"[15] The addition of this factor means that "the total return which a rational citizen receives from voting in a given election consists of his long-run participation value plus his vote value."[16]

In deciding whether or not to vote, rational individuals must compare this total benefit of voting with their estimate of the cost of voting. If the cost is less than the expected benefits, individuals will vote. If the estimated cost exceeds the expected benefits, they will abstain. This formula is sufficient to guide the voting behavior of the rational individual.[17]

A flow chart summarizing this decision is set out in Figure 1. This chart outlines the calculus of the voter who perceives differences between the parties in a simple two-party electoral contest. In calculating a voter's party differential, we subtract the utility the voter expects to receive from a Republican government from that expected of a Democratic one. Accordingly, a "negative" party differential indicates support for the Republicans, whereas a "positive" party differential indicates support for the Democrats.

In addition, we can express the Downsian voter's calculus in the form of an equation:

$$(p_1 \times |D|) + (p_2 \times L) - C = U \qquad (1)$$

where:

p_1 = the probability of an individual's vote making a difference in the outcome of the election,

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[12] Downs, p. 244.

[13] Ibid., p. 85.

[14] Ibid., p. 100.

[15] Downs, p. 270.

[16] Ibid.

[17] There is one exception to this conclusion: when individuals have a large enough long-run participation value to make their vote value greater than their estimated cost of voting, though they perceive no significant differences between the parties. Downs assumes that such individuals will actually vote, and he includes a detailed discussion of tie-breaking procedures for such voters. Given our operationalizations, the number of individuals perceiving no differences between the parties is extremely small in the sample we use. In addition, it is not possible successfully to operationalize the tie-breaking procedures suggested by Downs on the basis of the SRC data. Therefore, we have chosen to ignore this issue in the work reported in this essay.

D = the expected party differential,

p_2 = the probability that one's vote will make a difference in the "survival of the democratic, electoral system,"

L = the long-run participation value,

C = the cost of voting, and

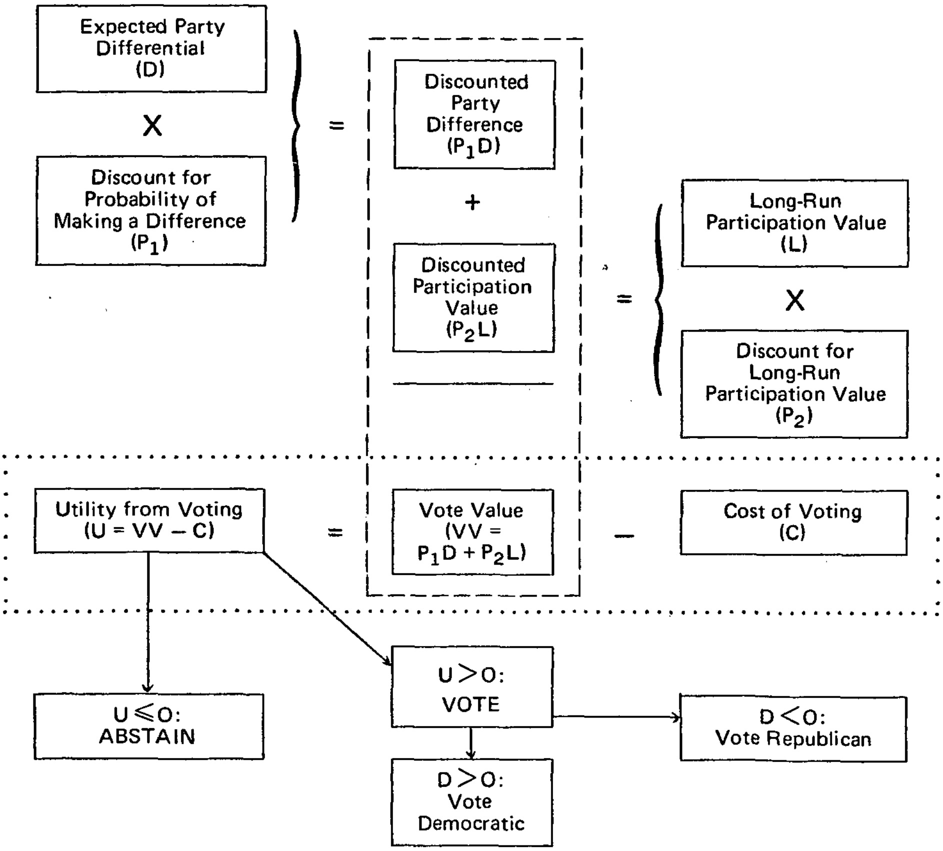
U = the net value an individual associates with voting.

Whenever $U > 0$ in this equation, the individual will vote.

Research Design

With these ideas in mind, let us consider the data and procedures needed to test the Downsian model. Ideally, we should have measures of each individual's perceptions of: differences between the parties on relevant issues, the costs of information, the attractiveness of party labels, the efficacy of voting, the value of the democratic system, and the cost of voting. The data should also include information concerning the actions of the individual in the election: whether one votes or abstains and, if one votes, the party or candidate chosen. Therefore, the data should stem from a panel study involving both preelection and postelection questions. Ideally, it should also involve a two-party contest for only one office.

Unfortunately, there is a dearth of available data conforming to these requirements. Few elections involve only one office, and those that do have not been widely or deeply surveyed. The most extensive and systematic panel studies concerning electoral behavior are the SRC's studies of postwar American presidential elections. These elections always involve many offices. Thus, voters who might abstain on the basis of the estimated benefits of voting in the


Expected Party Differential (D)
X
Discount for Probability of Making a Difference (P1)
=
Discounted Party Difference (P1D)
+
Discounted Participation Value (P2L)
=
Long-Run Participation Value (L)
X
Discount for Long-Run Participation Value (P2)
Utility from Voting (U = VV – C)
=
Vote Value (VV = P1D + P2L)
–
Cost of Voting (C)
U≤O: ABSTAIN
U>O: VOTE
D<O: Vote Republican
D>O: Vote Democratic

[a]Assuming that the expected party differential (D) ≠ 0.

Figure 1. Schematic Interpretation of Downsian Voter Rationality[a]

presidential race, may decide to turn out and cast a vote when they take the other contests into account. Furthermore, the surveys, which have not been designed to test theories based on the assumption of rationality, have substantive failings. For example, the questionnaires have never included explicit measures of the cost of voting. Similarly, though the surveys have included various indicators of party preference, little attempt has been made to measure the strength of party preference.

Despite these obvious difficulties, the SRC surveys are the best available source of voting data for tests of the Downsian model. Nevertheless, not all SRC surveys are equally attractive for our purposes. Only in 1960, 1964, 1968, and 1972 did the survey instruments allow individuals to identify the issues they considered of greatest importance. Yet these questions are needed for a straightforward operationalization of party differential. Furthermore, the 1968 election was a three-way contest, and the data from the 1972 survey became available only after this project was well underway (also the relevant questions were asked of only part of the 1972 sample). Hence, we use the 1964 SRC survey.

Our basic research strategy is to obtain measures of each of the component terms in Downs' voting calculus (cf. equation 1). We then combine these measures in a computer simulation that approximates the hypothesized thought processes of the voter. Consequently, we predict (1) whether the individual will cast a vote or abstain, and (2) the party the individual will favor. Finally, we compare these predictions with the reported voting behavior of the same individuals.

This research design is similar to the design employed by Shaffer. He also used a computer simulation of the voters in the 1964 SRC survey. As a test of the Downsian model, however, Shaffer's analysis falls short in two critical ways. First, his operationalizations of several of the major theoretical terms in the Downsian model are fundamentally flawed. Second, given both the simultaneous occurrence of several electoral contests and the absence of any direct measure of the cost of voting in the SRC data, it is necessary to augment the methodology of computer simulation to test the Downsian model.

Shaffer's operationalizations leave a great deal to be desired at a number of points. For example, he operationalizes the cost of voting by measuring the amount of information an individual acquires: the greater the information, the higher the cost.[18] But this certainly bears little or no resemblance to Downs' concept of the cost of voting. It may well be (as Shaffer argues) that the total cost of the information individuals acquire is proportional to the amount of information they obtain. But to argue that the cost of information constitutes a barrier to voting flies directly in the face of the Downsian conclusion that it is often rational to vote in relative ignorance. This confusion of participation costs with information costs casts doubt on all of Shaffer's conclusions concerning the issue of voter turnout. Thus, it is not surprising that the principal failing of Shaffer's simulation of the Downsian model is its inability to predict abstentions accurately: his simulation predicts 610 abstentions while only 282 of the 1352 individuals in his sample failed to cast a vote. As Shaffer himself puts it, "The percentage predicted to abstain is wildly incorrect."[19] Similarly, it is not surprising that the removal of this (faulty) measure of the cost of voting from the simulation leads to improved results.[20]

Other features of Shaffer's operationalizations are also unfortunate. To take another example, Shaffer relies primarily on the question concerning the expected impact of the election on the individual's personal finances to measure the differences between the parties or candidates.[21] Conceptually, this leads to serious problems. It leaves out much of what Downs had in mind in developing the concept of party differential. Even more importantly, less than half of the individuals indicated that party differences would have an impact on their personal finances. That is, the majority of the individuals in the sample did not perceive any significant differences between the parties with respect to the principal indicator Shaffer uses to measure party differential. On the other hand, in the case of those individuals who did perceive (even marginal) differences between the parties on the finance factor, Shaffer's simulation fails to give any weight to all other indications of issue positions. Hence, he places an arbitrarily narrow interpretation on the self-interest of 504 individuals, a procedure that cuts directly against Downs' position on the issue of self-interest.[22] Under the circumstances, we believe it is more in keeping with Downs' model to combine a measure of the "financial factor" with indicators of other issue positions for an operationalization of party differential.

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[18] Shaffer, *Computer Simulations*, pp. 58, 77, and 88.

[19] Ibid., p. 84.

[20] Ibid., pp. 88–94.

[21] Ibid., p. 70.

[22] Downs, in contrast, asserts, "There can be no simple identification of 'acting for one's own greatest

Problems of operationalization, however, do not constitute the only shortcoming of Shaffer's tests of the Downsian model. Any reasonable procedure designed to overcome the lack of empirical indicators of the cost of voting by introducing well-specified assumptions makes it necessary to go beyond the basic simulation techniques that Shaffer employs. Specifically, we have found it necessary to use regression techniques as a supplement to simulation to achieve a systematic test of the Downsian model. The exact reasons underlying this conclusion will become clear as we lay out the details of our tests below.

Summing up, while Shaffer has initiated an important line of analysis, his work does not constitute a clearcut or systematic test of Downs' theory. Accordingly, there is no reason to conclude that his mediocre results have much significance for the validity or relevance of the Downsian theory.

Operationalizing the Model

Much has been written about the process of operationalization; philosophers of science have been particularly articulate in their discussions of this subject. In operationalizing Downs' variables we have tried to follow proposals put forward originally by Carnap and espoused more recently by Hempel among others.[23] The essential idea is to replace theoretical terms with partial substitutes, to serve as surrogates for the theoretical terms under specified test conditions.

Calculation of Party Differential (D). Operationally, an individual's party differential is composed of five separate factors, designated symbolically as follows: I(1), I(2), I(3), F, and P. The first three factors represent the individual's perceptions and evaluations of the three most important issues in the campaign. The F factor stands for the individual's estimate of the financial implications of the election for his (or her) family, and the P factor represents the individual's (discounted) party identification. These factors are employed to measure the perceived impact of the election on welfare.

1. The Most Important Issues: I(1), I(2), and I(3). Respondents are asked to indicate what they feel are the first, second, and third most important problems the government should attend to (SRC variables 36, 41, and 46). Respondents are also asked what degree of concern they have about each of these issues (SRC variables 37, 42 and 47), and which party is most likely to do what is best for them on each issue (SRC variables 40, 45, and 50).[24] These responses form the basis of variables I(1) to I(3) as they enter into individuals' calculations of their party differential.

The responses enter into the individual's calculations in the following way:

a. Each issue, W(i), is given an a priori weighting, w(i), designed to reflect the relative importance of that issue. The most important issue identified is given the highest weight, the least important issue the lowest. The values assigned are:

$$W(1) = 4; W(2) = 3; W(3) = 2.$$

b. The individual's degree of concern regarding each issue (variables 37, 42, and 47) is then represented by a weight C(i), where the highest articulated concern is given the highest weight, and the lowest degree of concern the lowest weight:

C(i) = 1 (if the respondent indicates extreme worry),
C(i) = .5 (if the respondent indicates (s)he is fairly worried),
C(i) = .2 (if the respondent indicates (s)he is just interested).

c. Individuals' estimations as to which party is better for them on each issue are represented by P(i), where:

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benefit' with selfishness in the narrow sense because self-denying charity is often a great source of benefit to oneself. Thus our model leaves room for altruism in spite of its basic reliance upon the self-interest axiom" (p. 37).

[23] Carl G. Hempel, "The Theoretician's Dilemma," in *Aspects of Scientific Explanation* (New York: Free Press, 1965), esp. pp. 187–89.

[24] An unfortunate failing of the SRC survey instrument is that respondents are not asked which candidate, rather than which party, is preferable on each issue. Recent research indicates that the agreement between preferred candidate and preferred party is not so great as to render the issue trivial. See the discussion of party and candidate images in Samuel L. Popkin, John W. Gorman, Charles Phillips and Jeffrey A. Smith, "comment: What Have You Done for Me Lately?: Toward an Investment Theory of Voting," *American Political Science Review*, 70 (September 1976), 779–805.

P(i) = 1 (if the respondent believes the Democrats are better on issue i),
P(i) = −1 (if the respondent believes the Republicans are better on issue i).

d. Each issue enters into respondents' calculations of their expected party differential as the product of these three terms. Thus:

$$I(i) = W(i)C(i)P(i).$$

2. *Personal Finances Factor (F).* Respondents are asked whether they believe the election's outcome will affect their financial situation and, if so, to indicate which party is preferred (SRC variable 133). This factor is then assigned a value by multiplying a respondent's estimate of the election's financial impact by a party directionality indicator (identical to P(i) above). If respondent sees very important differences between the parties in these terms (responses 10 and 70), F is weighted 7; if important differences (responses 20 and 60), the weight is 4; if minor differences (responses 30 and 50), F is weighted 1.5.

3. *Party Identification (P).* As indicated above, Downs asserts that voters will often find it rational to stay uninformed about the issues in a campaign. In the absence of issue-oriented information voters can use cost-saving shortcuts such as party labels to decide their voting preferences. Downs would argue that the less issue information voters have, the more important is party identification in their voting decision. To operationalize this notion, we constructed an index to reflect the amount of the voter's information. This index was employed to measure the impact of party attachment on the voter. The more information, the more discounted a voter's party identification was in the equation predicting voting.[25]

The party identification score is based on each respondent's subjective party identification (SRC variable 146). Voters' responses to this question are given values from +3 (strong Democrat) to 0 (independent) to −3 (strong Republican). The individual's party identification score is then multiplied by an information factor so that the more information the respondent has, the smaller the product of party score and information. The information factor is operationalized as the respondent's *average* score on the media usage variables, with responses weighted as follows:

1. newspapers (variable 279): "How often did you read newspaper articles about the election?"—regularly = 4, often = 3, from time to time = 2, once in a great while = 1, reads no newspapers = 0;
2. radio (variable 281): "How many programs about the campaign did you listen to?"—good many = 4, several = 2.5, just one of two = 1, none = 0;
3. magazines (variable 282): "How many magazine articles about the campaign. . . ?"—same as radio;
4. television (variable 284): "How many TV programs about the campaign. . . ?"—same as radio.

The average media score divided by four is then subtracted from 1 in calculating the information factor. That is, the information factor = (1 − ¼ average media score). The party identification factor (P) is then operationalized as the product of the party identification score and the information factor.

4. *Party Differential (D).* An individual's party differential (D) is the sum of the five factors described above:

$$D = \sum_{i=1}^{3} I(i) + F + P. \qquad (2)$$

The sign of the party differential is used to predict the direction of individuals' votes, i.e., the party or candidate they will favor.

At this point, let us compare this operationalization of party differential with the standard set forth at the beginning of this section for the evaluation of any operationalization. It would seem certain that any individual who notes differences between the parties along the dimensions tapped by the operationalization will have a perceived party differential. But this observation glosses over several important issues. For example, any given individual may be concerned about other dimensions, untapped by this operationalization. Furthermore, individuals may well differ among themselves concerning the "proper" weights for the factors included in party differential. Perhaps most importantly, the SRC's survey data has no indicator of *the size* of perceived differences between the parties on any of the three most important issues. Therefore, our operationalization of party differential neither guarantees that all party differences perceived by individuals will be measured under test conditions nor insures against the ascription of some party differential where none actually exists.

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[25] Alternative operationalizations of this effect are possible. It would be feasible to increase the weight of issues to reflect increased information or to combine this approach with an information discount on party identification. Although we focus exclusively on the discount of party identification in our basic model, we have undertaken some tests employing alternative operationalizations for this factor.

Discounting Party Differential. To measure discounted party differential, each respondent's expected party differential is multiplied by a term representing the voter's estimated probability that the vote will make a difference. Discounting factors were computed from the respondents' estimates of the closeness of the election (SRC variable 19) on the basis of a procedure outlined in the Appendix. Higher subjective probabilities of making a difference are ascribed to individuals who think the election will be close than to those who feel the election will be one-sided. The specific probabilities employed are set forth in the Appendix. Since there is no a priori scale on which to measure the cost of voting, it is the ratios between these probabilities and not their absolute size which is relevant to our concerns.

Individuals' expected party differentials are simply the product of their expected party differential and their probability estimate of making a difference:

$$p_1 \times D = p_1 [\sum_{i=1}^{3} I(i) + F + P]. \qquad (3)$$

Long-Run Participation Value (L).

1. In operationalizing this term, we used SRC variables 390–393: the respondent's feelings as to how much attention the government pays to what the people think, how much political parties and having elections make the government pay attention, and how much attention members of Congress pay to the people who elect them. We constructed an index from these variables where the value of the index is the respondent's *average* score on the questions answered and where the answers are: a good deal = 2, some = 1, and not much = 0.

2. To compute a discounted version of long-run participation value, it would be necessary to weight this index by an individual's subjective estimate of the likelihood that one's vote will make a difference in saving the democratic system. Since there is no indicator of this factor in the SRC survey data, no individualized values for this discount factor were available. The best that could be done was to assign the same discount factor to each individual. But if each individual has the same discount, the absolute size of this discount serves merely as a weighting of the long-run participation value vis-à-vis discounted party differential. Nevertheless, since some starting weight was needed for our tests, voters were assigned a factor of 1/70,000,000 to discount their effect on saving the system. The relative size of this discount (versus the probability of making a difference to the outcome of the vote) determines the relative weights of party differential and citizen duty in the voter's decision to vote or abstain.

Vote Value. Vote value is simply the sum of the absolute value of the discounted party differential (p_1 x D) and long-run participation value (p_2 x L).

Cost of Voting and Turnout Predictions. Once vote value is computed from the data, the prediction of whether or not individuals will vote follows from a comparison of their vote value and their cost of voting. Unfortunately, there is no information on the cost of voting in the 1964 election study. Therefore, to make predictions about voting, we introduce assumptions about the cost of voting. Specifically, we work with three different assumptions about the cost of voting: (1) all individuals have an equal cost of voting; (2) the cost of voting is randomly distributed among voters and this distribution is flat; and (3) the cost of voting is randomly distributed among voters and this distribution is lognormal. Both the operational meaning and the empirical implications of each of these assumptions are discussed in detail as they are brought into the analysis in later sections of this article.

The Basic Model's Predictions

As indicated above, the two most immediate predictions of Downs' model are whether given individuals will actually vote and which party they will support. Equations 1 and 2, composed of the independent variables I, F, P, L, and the probability discount (efficacy), generate these predictions. Table 1 compares the predicted and actual party preferences of individuals.

As an initial indication of the accuracy of the model, notice that 924 of the 1,067 individuals' predicted preferences correspond to their actual preferences. The model is correct on 86.6 percent of the individuals. But to evaluate the model, we should compare the model's predictive accuracy with the accuracy of a random guess technique. Such a technique would use the percentages of actual Democratic and Republican preferences to predict individual preferences (randomly). Using these marginals one would predict an individual to prefer the Republicans with a probability of .307 (the percentage of actual Republicans in the sample) and the Democrats with a probability of .693. The technique would lead to 57.4 percent correct predictions. Thus, the model's predictions are better than predictions based on random use of the marginals. But *how much better than random* are our predictions? In this instance the model correctly predicts 29.2 percent more of the total cases than the random prediction does. To reflect this we can develop a statistic (hereafter called R^{*2}), representing how much better than random the model predicts. The general formulation of the statistic is as follows:

$$R^{*2} = \frac{\begin{array}{l}\text{\% correctly}\\ \text{predicted}\\ \text{by model}\end{array} - \begin{array}{l}\text{\% correctly}\\ \text{predicted}\\ \text{by random}\end{array}}{100\% - \begin{array}{l}\text{\% correctly}\\ \text{predicted}\\ \text{by random}\end{array}}.$$

We will employ R^{*2} throughout this article. In the case discussed in this paragraph, for example, the Downsian model correctly predicts the preferences of 86.6 percent of the individuals and explains 68.5 percent of the random predictor's errors.

But, as indicated above, it is possible to utilize additional tests of the predictive accuracy of the Downsian model of voting. Two basic sources of error exist in any experiment: inaccuracy of measurement and faulty operationalizations. The latter can lead to the omission of important data and the former to distortion of the data. A few simple assumptions about the distribution of these errors and missing data make it possible to perform an additional, indirect test of the Downsian model. If the size of these errors (measured in terms of their importance relative to existing party differentials) *is uncorrelated with existing party differentials* and distributed randomly (in a specified fashion) with respect to its pro-Democratic and pro-Republican content, we can derive some testable implications about the errors in our simulation.

Assume first that the distribution of errors and unrecorded information over the Democratic-Republican continuum is flat. This means that there is an equal likelihood of a given bit of missing information being neutral, mildly pro-Democratic or Republican, moderately so, substantially so, or sufficiently pro-Democratic or Republican to outweigh the impact of all other factors combined. Assuming that the size of a given error cannot be greater than the maximum party differential that can occur in our simulation, we can make the following prediction about errors. The lower an individual's party differential as measured by our data, the greater the probability that missing data will change that individual's party preference. Thus the lower an individual's party differential, the greater is the probability that the simulation will yield an erroneous prediction of the individual's party preference. Moreover, this tendency should be linear. If we group voters into deciles in terms of increasing absolute values of their party differentials and examine the simulation's predictions concerning party preference across these groups, we should find the accuracy of the model's predictions increasing in a linear fashion as we move to groups with higher party differentials.

To test this prediction, we performed a linear regression on the relevant data. This test produced a positive relationship with $r = .7699$ ($r^2 = .5927$).

Note, however, that alternative assumptions about the distribution of errors of differing sizes can be used. For example, another assumption (suggested by its frequent occurrence in important econometric models) is based on the supposition that the distribution of sizes of the bits of unrecorded information is lognormal with respect to the party differential continuum. The use of this bridge principle leads to the conclusion that the relationship between increasing absolute value of party differential and the accuracy of the simulation's predictions of party preferences should be logarithmic. This means that the relationship between the logarithm of the absolute value of party differential and percent correct predictions should be linear. The execution of this test in a fashion similar to that described in the preceding paragraphs yields a positive relationship with a Pearson's r of .9064 ($r^2 = .8216$).

Both these results lend credence to the Downsian model as we have operationalized it. Which of the assumptions about the shape of the distribution of unrecorded data is more descriptively accurate is an interesting question. Though the analysis reported here does not allow a definitive conclusion, it tentatively suggests that the lognormal distribution is more accurate.

Although the model and the data permit an immediate test of the prediction of voters' party preferences, no such immediate test of turnout is permitted. The difficulty stems from the absence (in the survey) of a measure of the cost of voting. Without such a measure we cannot predict whether or not a given individual will turn out. The Downsian hypothesis, after all, bases the turnout decision on a comparison of vote value and cost (equation 1).

In the absence of an independent measure of cost, we have to introduce some assumption about the distribution of costs among the voters to generate predictions about turnout. If cost is randomly distributed relative to vote value, then the higher an individual's vote value, the more likely it is to exceed that individual's cost of voting. Thus the individual is more likely to turn out to vote. Using this argument, we can generate predictions of turnout by introducing particular distributions of cost over the sample. We tested three different assumptions about the distribution of costs: (1) the case in which everyone is assumed to have the same cost of voting, (2) the case in which every cost (from zero to a cost equal to the highest possible vote value) is equally probable, and (3) the case in which cost of voting is assumed to be lognormally distributed relative to vote value. Each of these generates a different set of predictions about who will vote. We can then compare the predictive accuracy of the model under each of these assumptions.

The theory predicts that individuals will vote if, and only if, their vote value exceeds their cost of voting. If we assume that everyone in the sample has an identical voting cost (C), those individuals with a vote value greater than C can be predicted to vote, and those below to abstain. Thus, if X percent of the electorate doesn't vote, then *they* are the individuals whose values are in the bottom X percent of the vote-value distribution. To test this we can rank-order the individuals in ascending order according to their vote values; set the cost of voting equal to the vote value of the individual at the X percent level of the vote-value ranking; and predict that those above the individual will turn out and those below will abstain.[26] In our sample, 1,000 of the 1,100 registered voters or 90.9 percent voted in 1964.[27] The procedure outlined above therefore set the cost of voting

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[26] Actually, to eliminate an indeterminate prediction with respect to that one individual, the cost of voting is set just above that individual's vote value and below that of the next higher individual in the rank ordering.

[27] The sample size here is larger than that used in predictions of party preference; in our analysis of party preference, we dropped individuals with D = O for whom we could not make predictions. Other fluctuations in sample size stem from similar considerations.

Table 1. Predicting Party Preference

Individual's Actual Party Preference	Individual's Predicted Party Preference		
	Republican	Democratic	Row's % of Sample
Republican	268	60	30.7
Democratic	83	656	69.3

N = 1067
% correctly predicted by model = 86.6
% correctly predicted by random = 57.4
$R^{*2} = .685$

Table 2. Testing the Basic Model's Turnout Predictions with A Priori Weights and Different Assumptions About Cost of Voting Distributions

	Assumptions re Cost of Voting Distributions		
	1 Equal Cost Assumption	2 Flat Cost Distribution[a]	3 Lognormal Cost Distribution Engle Curve Analysis[a]
R^{*2}	.0650	inapplicable	inapplicable
r	inapplicable	.8891	.9206
r^2	inapplicable	.7905	.8474
Range of turnout rates over vote value range	.00 – 1.00 (only 2 categories)	74.1% – 98.1%	74.1% – 98.1%

[a] Using only 8 points because the top 30 percent of the vote-value scale was collapsed due to small n's.

between the vote values for individuals 100 and 101 from the bottom.

When all individuals from 101 up are predicted to turn out and all below are predicted to abstain, we obtain predictions that are 84.6 percent correct. Although on the face of it this might appear to be a substantial result, the skewed distribution of voters and nonvoters calls the strength of these predictions into question. For if we were to predict turnout on the basis of a random prediction based on the marginals, we could predict 83.5 percent of the sample correctly. This yields an R^{*2} of only .065. In other words, using the equal cost assumption, we can explain only 6.5 percent of the cases left unexplained by a random estimate based on the marginals (see Table 2).

This poor result calls into question the descriptive accuracy and acceptability of the "equal cost" assumption. It is unlikely that all individuals bear the same cost of voting. Moreover, that assumption requires that we break the sample into two groups and predict turnout rates in those two groups. This dichotomy loses a great deal of information. The other two assumptions of random cost distribution do not impose this dichotomy upon our predictions and permit more powerful tests.

What happens when we assume that voters have an equal probability of any cost (i.e., a flat distribution)? Now Downs hypothesizes that the probability of a given voter's turning out is the same as the probability of the voter's vote value exceeding the cost of voting. The higher the vote value, the greater the probability that the voter will turn out. Equal increments in vote value should yield equal increments in probability of turnout. Given this assumption, voters can be ranked in order of increasing vote value and broken into deciles of increasing vote value. Each higher decile of vote value should have a higher turnout rate. If the individuals were evenly distributed over the range of vote values, we could predict a linear relationship between the mean vote value in each decile and the turnout rate in each decile. Unfortunately, the situation is complicated by the normal distribution of individuals across the range of vote values and by a scarcity of individuals at the high end of the range. In particular, the top three deciles together contain only 53 individuals. Thus, to insure a sufficient number of individuals in each category these three deciles were collapsed into one, and the prediction of turnout was based on an eight-point fit.

We regressed the mean vote value in each category against the percentage turnout in each category to test for the linear relationship between vote value and turnout rate. The results are displayed in Table 2. The Pearson's r is .889 ($r^2 = .791$), which indicates a substantial relationship between vote value and turnout. Moreover, the range of the mean turnout rates across the vote value categories is 74.1 to 98.1 percent, indicating a relatively strong relationship. Thus, the introduction of a seemingly more realistic distribution of costs produces a prediction in which 79.1 percent of the variance in turnout rates is explained. Notice that using the best random predictor (i.e., predicting the mean turnout rate) in the flat distribution model would yield an r^2 of .00. Thus, the r^2 of .791 can be favorably compared with the R^{*2} of .065 of the equal cost model. Although one could argue that the results of this test are not directly comparable to the results of the equal cost assumption because of the different statistical tools used, it is clear that the flat distribution of cost model yields predictions that are substantially more in agreement with the facts. Consequently, the flat distribution of cost model can be interpreted as disconfirmation of the equal cost assumption.

But just as we were led to question the descriptive accuracy of the equal cost model, it is possible to raise questions about the flat cost distribution assumption. As an alternative assumption about the distribution of costs, we can consider the predictions that would be generated by assuming costs to be distributed in a lognormal fashion.[28] If we assume that costs of voting are independent of vote value and lognormally distributed, we can perform a log transformation of the vote value scale and break the scale into deciles as before. We can then predict a positive linear relationship between the mean in each decile of logged vote value and the probability of voting. The results of performing these operations are reported in Table 2. The regression yields strong confirmatory results. Pearson's r and r^2 are .921 and .847 respectively. Almost 85 percent of the variance is explained. Since random predictions in this case would have yielded an r^2 of .00, we conclude that utilizing our a priori weights and a lognormal distribution of costs assumption, our simulation corroborates the Downsian model's predictions of turnout.[29]

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[28] A technique called Engel Curve analysis has been developed in econometrics to deal with decision problems analogous to the problem of whether or not to vote. The economic problem for which this technique was developed is the prediction of household purchases of consumer durables. The household's decision is assumed to be a function of household income. Each household is assumed to have some income threshhold level for the purchase of the durable. If the household's income exceeds the income threshhold level, the household is predicted to purchase the durable. This technique permits the formulation of probabilistic predictions regarding the household's purchase of the item. For a given probabilistic distribution of income level thresholds, the probability that a given household will purchase the item is calculated as the probability that its income is above the income threshold level. Empirically it has been found that the best predictions are obtained when income threshold levels are assumed to be distributed lognormally and independently of income. This assumption yields the prediction that the relationship between the log of income and probability of purchase is linear.

With respect to the question of voter turnout, we can adapt the assumptions of this model by letting cost play the role of income threshold levels and letting vote value play the role of income. For a good discussion of the nature of Engel Curve analysis see J. S. Kramer, *Empirical Econometrics* (New York: North Holland, 1971), Ch. 3.

The results of the tests so far allow us to draw a number of conclusions. First, with regard to party direction, the Downsian model as operationalized performs sufficiently well ($R^{*2} = .685$) to justify confidence in further use of the basic model. Secondly, our bridge assumption of an equal cost of voting for each voter seems both empirically limited and a poor predictor when coupled with our model ($R^{*2} = .065$). Its preliminary rejection seems warranted. Thirdly, the assumption of a flat distribution of costs yields better predictions of turnout than the equal cost assumption ($r^2 = .791$). But the analysis assuming a lognormal distribution of costs performs better still in predicting turnout ($r^2 = .847$) leading us to believe that it is a better description of the actual cost distribution. Accordingly, we tentatively accept this assumption concerning the distribution of costs.

Further Considerations

The tests reported in the preceding section lend support to many aspects of the Downsian model of individual voting behavior. Nevertheless, these results may fail to convince the reader of the utility of the Downsian model. After all, some models may do equally well or even better in predicting the directionality of the vote with comparable data[30] and others may do better predicting turnout. But to denigrate the importance of the Downsian model for these reasons would be to miss one of the main points of this study. The model tested here makes *both* kinds of predictions. Therefore, it is a more falsifiable (or powerful) theoretical construct than any collection of *ad hoc* hypotheses designed to "explain" a single data set. Moreover, the Downsian model rests on explicit behavioral assumptions (e.g., rationality) which have testable implications in other contexts. Accordingly, Downs' model of individual voting behavior fits into a broader framework and permits cumulative progress.

Nevertheless, a series of questions about the Downsian model and the testing procedures we have employed remains. First, do changes in the parametric weights of the variables affect the accuracy of the predictions? Second, can some proper subset of the variables in the model predict as well as or better than the full model? If so, does simplicity demand the acceptance of this subset in preference to the complete Downsian model? Third, does the model predict equally well for all subgroups of individuals? If it does not, either the Downsian model itself is rendered suspect, or we must question the generality of the operationalizations.

Adjusting Parametric Weights. The specific values assigned to the variables in our a priori model rest on judgments that are ultimately arbitrary. Therefore, they are subject to serious question and could possibly be changed to increase the model's predictive accuracy. Indeed, given the aggregation problems associated with a model of this sort, even the ordinal properties of the variables' weights can be questioned.[31]

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[29] The high correlation obtained here suggests that it would be desirable to conduct independent tests of the assumption that voting costs are distributed lognormally.

[30] See, for example, Stanley Kelley, Jr. and Thad W. Mirer, "The Simple Act of Voting," *American Political Science Review*, 68 (June 1974), 572–91.

[31] To see this consider the following illustration. There is no a priori justification for assigning a stronger explanatory role to one voter's response that (s)he is a "strong party identifier" than to another's response that (s)he is a "weak party identifier." In fact, there are significant variations in language usage among ethnic and other subgroups in American society. Members of some subgroups may be prone to overstatements and the use of strong language to describe what are actually rather weak preferences (the stereotype of the brash Texan comes to mind in this connection). Members of other groups may characteristically understate strong preferences (the stereotype of the taciturn Vermonter constitutes an example). Such phenomena may be nonrandomly distributed with respect to the variable labeled party identification in our model. To take an extreme example, if all and only Texans were in reality weak party identifiers and all and only Vermonters were actually strong party identifiers, then weightings on this variable derived from the statements of respondents would be in exactly the wrong order. To the extent that weaker versions of such phenomena are

Since there are, in principle, an unlimited number of possible parametric weights, in the absence of theoretical arguments, it seems to us that justification for any set must rest on the criterion of empirical fit. Unfortunately, even this criterion is ambiguous in the case at hand. We have already employed three different indices of empirical fit: R^{*2} to measure the accuracy of predictions about party direction, the analysis of errors in the party-direction predictions, and the analysis of predictions regarding turnout. It is difficult to find adjustments in parametric weights which improve prediction along all three of these dimensions simultaneously.

Even more striking, there exists no practical statistical technique to estimate best weights for the parameters which would maximize predictive accuracy. In particular, note that *regression is not an appropriate technique to generate parametric weights that maximize the number of correct predictions.* To see this, consider how the regression technique operates when the dependent variable is binary. Regression minimizes the *aggregate* difference between predicted values and actual values. In fact, maximization of the number of correct predictions is not considered in this model. This means that a regression to predict direction of the vote would generate coefficients to group Democratic voters as close to one another as possible and would do the same for Republican voters. Ideally, the regression would attempt to assign coefficients to give all Democratic voters a single numerical party differential and all Republican voters another numerical party differential. As a technique for adjusting parameter weights in the Downsian model, then, regression has two important defects. It distorts the underlying theory which presumes voters to have different party differentials, and it maximizes the wrong thing.

Moreover, we were unable to find any other feasible technique which could do better than regression in adjusting parameter weights. Certain linear programming models are applicable in principle, but their costs are prohibitive (estimates ran as high as several solid days of computer time). No systematic "best fit" technique appears available.

Nevertheless, to get some indication of the model's sensitivity to changes in parametric weights, several of them were adjusted in an ad hoc fashion. The predictions resulting from these changes can be compared with those obtained using the a priori weights. Since the range of possible adjustments is limitless, we have had to be extremely selective in preliminary sensitivity testing. However, initial tests do permit several conclusions.

Improvements are definitely possible, at least within the data set employed in this study. (To establish the status of these improvements it would be necessary to use them comparatively in the context of at least one other data set.) Manipulations of the relative weights of party differential and long-run participation value, of the efficacy intervals, or of issue importance can produce marginal improvements along *some* of the dimensions of empirical fit. Combining adjustments also permits slight improvements.

On the other hand, our preliminary tests suggest that the ordinal properties of the scales are less open to improvement. Thus, all reversals of the efficacy scale (i.e., giving people a higher incentive to vote as they perceive the election to be more one-sided) produced deteriorations in empirical fit. Similarly, inverting the information scale led to a marked worsening of predictive results. At the same time, marginal changes in both of these scales which preserved their ordinal characteristics produced little change in the empirical fit of the model.

Simplified Models. To determine whether a simplified version of the model might yield equally good or even better predictions, we carried out two kinds of tests in a preliminary fashion. One type involved comparing predictions derived from the use of single variables with those stemming from the full Downsian model. The other type focused on the omission of single variables and its effect on predictions of both the directionality of votes and turnout.

Unfortunately, it proved difficult to make direct comparisons between the results emerging from these manipulations and the results reported earlier. Though single variables sometimes yielded good predictions with respect to one or the other of the dependent variables, these predictions were frequently restricted to a small subgroup of the overall sample. For example, the finance factor, treated as the sole independent variable, is a good indicator of ultimate party preference *for the subset of individuals who perceived differences between the parties along this dimension.* To compare the relative predictive power of the full model and the financial factor, however, we must examine the predictions produced by the full

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present in our data set the ordinal properties of the weights assigned to the variables in the model are suspect.

model for the reduced sample of individuals who perceived differences between the parties on the financial factor. Whereas the financial factor alone correctly predicts party direction in 92.8 percent of the cases in the reduced sample (n = 361), the full model predicts 94.2 percent of the cases accurately *in this sample*. And of course the full model permits predictions of turnout as well, which the financial factor alone cannot do because of the small sample size. Similar conclusions held on the tests of other single variables.

The problem of comparability is not nearly as severe when only one variable is omitted from the full model. Sample size was not substantially reduced by such procedures, and so the predictive accuracy of both the full and reduced models on turnout and direction could be compared. It turns out that dropping individual variables reduces predictive accuracy in most cases. In one case of particular interest, the omission of the efficacy variable (i.e., the indicator of the voter's subjective likelihood of affecting the outcome) reduces the model's ability to predict turnout significantly (see Table 3). This raises questions about the recent argument of Ferejohn and Fiorina, who conclude that closeness of the election is unimportant.[32] A more surprising result is that dropping the information variable has very little effect on the ability of the model to predict turnout (see Table 3). This result raises serious questions about either our operationalization of the information factor or the explanatory role of this factor in the Downsian model.

Subgroup Analysis. Our analysis of the predictions produced by the basic model turned up certain indications that Downs' theory fares better for some groups of individuals than for others. For example, our Downsian model is better at predicting voting behavior of individuals who perceive the elections to have an impact on their finances. It also generates better predictions for those who can identify substantive issues than for those who cannot. These observations lead to fundamental questions concerning the Downsian model itself.

To examine issues that arise from the observation that some subgroups appear to conform more closely to the Downsian conception of individual voting behavior than others, consider the voting behavior of cross-pressured versus non-cross-pressured individuals. The cross-pressured individuals in the sample are those 168 persons who exhibit one or more inconsistencies in their responses concerning party preferences on the factors contained in the party-differential construct. One might suppose that our rate of error would be higher in this group due to the psychological impact of cross pressures on the behavior of these individuals. However, Downs' analysis contains no such hypothesis; in fact, such a hypothesis contradicts his formulation of the rational-choice calculus summarized in Figure 1. In Table 4 note that our R^{*2} is only .282 for the cross-pressured individuals in contrast to .685 for the total sample and .767 for the non-cross-pressured people (see Tables 4 and 5).

One possible hypothesis concerning these results is that some individuals (e.g., those who are cross-pressured) are not proceeding rationally in making their voting decisions. In our judgment, however, there is a more plausible hypothesis.

One possibility is that these anomalous results follow from a theoretical problem in the Downsian model. Downs assumes the existence

Table 4. Predicting Party Preference for Cross-Pressured Individuals

Actual Party Preference	Predicted Party Preference: Republican	Predicted Party Preference: Democratic
Republican	38	21
Democratic	34	75

N = 168
% right on the basis of random predictions = 54.4
% right on the basis of our predictions = 67.3
$R^{*2} = .282$

Table 3. Some Results of Simplification

	Basic Model	Basic Model with Efficacy Neutralized*	Basic Model with Information Neutralized*
r (turnout with Engel Curve)	.921	.828	.909

*Other weights adjusted so as to maintain relative importance of long-term participation value and party differential established in the basic model.

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[32] John A. Ferejohn and Morris P. Fiorina, "Closeness Counts Only in Horseshoes and Dancing," *American Political Science Review*, 69 (September 1975), 920–25.

Table 5. Predicting Party Preference for Non-Cross-Pressured Individuals

Actual Party Preference	Predicted Party Preference Republican	Predicted Party Preference Democratic
Republican	230	39
Democratic	49	581

N = 899
Hardcore errors = 88
% right on the basis of random predictions = 58.1
% right on the basis of our predictions = 90.2
R^{*2} = .767 (vs. .654 for Southerners only)

of unified parties competing for a single office, and all our operationalizations reflect this assumption. However, reality is more complex. Individuals may experience cross pressures with respect to a candidate even though they are not cross-pressured regarding party orientation. Moreover, the images of a given party may vary considerably from one region of the country to another.

To see the implications of such phenomena consider the following illustration. In 1964, there may well have been many white southerners who regarded themselves as strongly pro-Democratic (i.e., they gave no indication of being cross-pressured on party) but who believed that Lyndon Johnson did not represent the policy positions of their party. Some of these individuals may have chosen to vote for Goldwater as the candidate whose views were most compatible with the "southern" image of the Democratic Party. Such persons would *not* show up in our sample as cross-pressured individuals, but their voting behavior would be predicted incorrectly by our basic model. To evaluate this possible source of erroneous predictions we checked to see what proportion of our errors among non-cross-pressured whites were in the South in contrast to the rest of the country. While only 17 percent of the non-cross-pressured whites for whom we made predictions were southerners (144 out of 814), one-fourth of the 88 errors in question (i.e., 22 out of 88) occurred in cases of individuals living in the South.

What can we conclude from this illustration? At least some of the differences in the predictive results for various subgroups stem from structural problems built into the Downsian model, and our operationalizations, for example, the failure to distinguish between candidates and parties. There is also a distinct possibility that the voting behavior of some subgroups in the electorate differs significantly from that of other subgroups. In such cases, the weights associated with various factors in the Downsian model (heretofore assumed to be uniform across the whole sample) could be modified for specified subgroups to yield improved predictions. Note again that almost any modifications of this type would be compatible with the basic Downsian model; Downs never asserts that all individuals will weigh the factors included in the voting calculus in an identical manner.

Comparison With the SRC Model

It is possible to gain additional insight into the performance of the Downsian model by duplicating our analysis for a different, competing model. Such a test would serve two purposes. It would lay the basis for a rigorous comparison of the predictive performances of the Downsian model and another model by using the same sample and employing many of the same techniques. Further, it would yield evidence concerning the validity and reliability of the methodological tools introduced in this essay. The model that has evolved from the work of the Survey Research Center (SRC) is the obvious choice for comparison with the Downsian model by virtue of the prominence it has attained, the availability of relevant data, and the ease of operationalization.[33]

The basic elements in the SRC model are attitudes toward the visible objects of politics:

> In unraveling the causal threads leading to the vote we begin with the immediate psychological influences on the voting act. By casting a vote *the individual* acts toward a political world whose objects he perceives and evaluates in some fashion.[34]

In general, therefore, the SRC model deliberately shifts attention away from the group-oriented or sociological approach of earlier studies to what are believed to be more proximate causal forces in voting behavior. Nevertheless, this model retains an important element of the sociological approach by assigning a central role to party identification.

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[33] The SRC's research on voting began without any clearly specified model of individual voting behavior, though there was a commitment to a research strategy. Specifically, the SRC analysts set out to examine the role of psychological variables in addition to social characteristics. For an excellent review of the evolution of the SRC's model over time, as well as a perceptive critique of the model, see Peter B. Natchez, "Images of Voting: The Social Psychologists," *Public Policy,* 18 (Summer 1970), pp. 553–88.

[34] Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter: An Abridgement* (New York: Wiley, 1964), p. 13.

With respect to party preference the specific components of the SRC model are: (1) attitude toward the Democratic candidate as a person, (2) attitude toward the Republican candidate as a person, (3) attitude toward the parties and candidates relating to benefits to various groups, (4) attitude toward the parties and candidates relating to domestic policy, (5) attitude toward the parties and candidates relating to foreign policy, and (6) attitude relating to the general performance of the parties in handling the nation's affairs. The individual's score on each of these attitudinal dimensions is an integer representing the number of responses in each category which are elicited from a set of open-ended questions asking about specific evaluations of each party and candidate. Responses on each dimension are simply summed with pro-Republican scores weighted positively and pro-Democratic scores weighted negatively.

The scores on the six attitude dimensions are employed as the independent variables in a linear regression model which predicts party preference in the election. The dependent variable is the probability of voting Republican, p(R), which receives a value of one for Republican voters and zero for Democratic voters.[35] "The multiple correlation of the several predictors with party preference varied in the range of .72 to .75 over the four presidential elections [between 1952 and 1964]."[36]

Except for providing *prima facie* evidence that the attitude variables are highly correlated with party preference, this correlation coefficient by itself is not very interesting. Of greater interest is what the equation indicates about the roles of the various attitude factors. Because the dependent variable is dichotomous, it is possible to interpret the regression coefficients in probabilistic terms. That is, each regression coefficient is interpretable as the change in the probability of voting Republican resulting from a unit change in the associated attitude score. Furthermore, each of the regression coefficients can be multiplied by the mean value of the associated attitude dimension in order to provide a measure of the total impact of the attitude in question on the election (i.e., the net contribution of each attitude to the Republican percentage of the major party vote).[37]

Another important result of the regression analysis associated with the SRC model is the production of a set of weights for the individual factors which can be used to make determinate predictions concerning party preference. Thus, the attitude scores of individual voters can be weighted by the appropriate regression coefficients, summed, and added to the constant term to produce estimates of the probability of each individual preferring the Republican option in the election. However, an important assumption underlies this procedure. The use of systemic weights implies that every individual attaches the same importance to each of the attitude dimensions.

It is feasible to use the scores obtained through this procedure in a simulation of the election to make determinate predictions regarding party preferences. Such predictions may be made either by employing a random number generating process or by using a rule of thumb that assigns the party preference which would predominate over a large series of simulations. We have made use of the latter technique in the comparative results reported below. Thus, Republican party preferences were predicted for those with probability scores greater than .5 and Democratic party preferences for those with scores of less than .5.

The SRC does not claim that its model relating to party preferences is adequate to predict turnout behavior as well. Nevertheless, these analysts sometimes advance the argument that turnout is an increasing function of the intensity of party preference,[38] and the model of party preference is directly interpretable in terms of intensity. Because large numbers of individuals vote even with low intensity levels in the realm of party preference, the SRC argues that other factors play some role in the turnout decision. These include closeness of the election, sense of efficacy and sense of citizen duty. Though the SRC analyses include no explicit model of the turnout decision, it should be true *ceteris paribus* that turnout will

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[35] The equation estimated is:

$$p(R) = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_3 + \beta_4 x_4 + \beta_5 x_5 + \beta_6 x_6 + U$$

where p(R) is the probability of voting Republican, the X_i's are the scores on the six attitudinal dimensions, U is a random error term, and the β_i's are the regression coefficients.

[36] Donald Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March 1966), 28.

[37] The relative importance of the six attitude dimensions over a series of elections is examined using this procedure in Stokes and in Natchez.

[38] This argument is formulated and supported by data using an independent measure of intensity of party preference in Campbell et al., Ch. 4.

increase as a function if intensity, measured on the basis of the model of party preference. Unfortunately, the SRC has never reported a test of this hypothesis.

In published reports, the coefficients of the SRC model have been estimated only for those respondents who reported actually voting in the election. In carrying out the tests reported in this section, however, we have estimated the SRC model for both voters and nonvoters. The Downsian model, which predicts both party preference and turnout, is necessarily estimated for both voters and nonvoters; the estimation of the SRC model for a similarly constituted sample is necessary to permit a rigorous comparison of the predictive performances of the two models. Moreover, it is impossible to evaluate the performance of the SRC model in predicting turnout unless nonvoters are included in the sample. The coefficients of the modified SRC model for the 1964 election are reported in Table 6.[39]

Table 6. Coefficients for the SRC Six-Factor Model for the 1964 Election

Factor	β_i
Democratic candidate	.07091
Republican candidate	.04641
Group orientation	.04885
Domestic issues	.06143
Foreign issues	.08700
General management	.06433

$N = 1223$
$r = .74149$
$\beta_0 = .44193$

At this point, we employed the simulation procedures outlined above to obtain party preference predictions for the 1964 election using the SRC model. The results are reported in Table 7 along with the results previously obtained from the Downsian model. These results indicate that the two models are quite comparable with respect to their ability to predict party preferences; the SRC model holds a slight edge in predictive accuracy (less than 3 percent). The analysis of errors for the two models produces similar results. The correlation of errors with the predictor variable (r_{pty}) is .07 higher for the SRC model than the same measure for the Downsian model. In both cases, the relationship is in the hypothesized direction and is significant at the .01 level.

We also generated turnout predictions for the SRC model by treating individual scores on the SRC predictor variable as a measure of vote value and utilizing the lognormal cost assumption described previously in the discussion of the Downsian model. The results of this test are similar to the results of the test relating to party preferences. In this case, however, the Downsian model ($r = .921$) performs slightly better than the SRC model ($r = .880$). Again, the slopes are positive and significant at the .01 level. Of course, the coefficients for the Downsian model and the SRC model are not directly comparable since the SRC model does not include the cost of voting as a relevant factor and since only one element in the implicit SRC model of turnout is tested. Nevertheless, this evidence does provide partial corroboration for the hypothesis that the SRC model's measure of intensity of party preference accounts in part for decisions concerning turnout.

Further insights regarding the predictive performances of the two models emerge from an examination of the voting behavior of the subgroup of cross-pressured individuals. In each case, the sample includes those respondents exhibiting conflicting party preferences (excluding neutrality) on any pair of relevant factors. The coefficients of the SRC model for cross-pressured individuals in the 1964 sample are reported in Table 8. Predicted party preferences for cross-pressured persons generated by both the SRC model and the Downsian model appear in Table 9. For this subgroup the predictive performance of the SRC model is better than that of the Downsian model. Erroneous predictions concerning the behavior

[39] We estimated the SRC model for the subgroup of voters alone to compare the results with the total sample including both voters and nonvoters. Although the multiple correlation of the six factors with vote was smaller in the latter case, the difference was slight (approximately .03) and the coefficients of the two models were quite similar. A further modification of the sample resulted from the exclusion of nonregistered respondents, to facilitate comparison with the sample used in analyzing the Downsian model. The difference in final sample size for the two models is attributable to the exclusion of different cases due to missing data.

In addition, it should be noted that the factor scores and predictions for the six-factor model reported herein are peculiar to the particular coding scheme employed for translating responses to the open-ended questions into scores for the six factors. Coding schemes other than the one employed here may be equally justifiable. We believe, however, that the (probably marginal) differences which would result would not significantly alter the conclusions reached in this section. See, for example, Michael R. Kagay and Greg A. Caldeira, " 'I Like the Looks of His Face': Elements of Electoral Choice, 1952–1972," paper prepared for annual meeting of the American Political Science Association, San Francisco, September 2–5, 1975. These researchers report 89.5 percent accuracy in party predictions for 1964 (voters only), as compared to 89.1 percent reported in the text.

Table 7. Party Preference Predictions for the 1964 Election

	Actual	
Predicted	Democratic	Republican
A. SRC Model (N = 1223)		
Democratic	801	91
Republican	41	289
% correct party preference predictions = 89.1 $R^{*2} = .746$ $r_{pty} = .841$		
B. Downsian Model (N = 1067)		
Democratic	656	60
Republican	83	268

% correct party preference predictions = 86.6
$R^{*2} = .685$
$r_{pty} = .770$

of cross-pressured individuals accounted for about 70 percent of the predictive errors of the SRC model and about 38 percent of the predictive errors of the Downsian model. Unfortunately, the sample of cross-pressured persons for the Downsian model is so small that it is not feasible meaningfully to perform analyses of errors on party preference predictions and on turnout in this connection. On the other hand, the SRC model is a poor predictor of turnout for cross-pressured individuals (r = .249).

The following conclusions are justified on the basis of these tests. First, both the SRC model and the Downsian model are good predictors of the party preferences of individuals; there is little to choose between them in this regard. The high correlation between errors on party preference predictions and individual scores on the six factors of the SRC model is consistent with the interpretation of these scores as a measure of intensity of preference. Second, despite theoretical objections, there does not appear to be any great difficulty in using systemic weights for the components of the models to make determinate predictions of individual party preferences. However, the fact that the weights of the Downsian model were selected a priori whereas the SRC weights were estimated by applying a technique which minimizes the variance of the predictor variable around the dependent variable may account for the differences in the predictive performance of the two models with respect to individual party preferences. Third, both the Downsian model and the SRC model permit successful predictions of turnout within the framework of the Downsian model's assumptions concerning the turnout decision. This lends additional support to the interpretation of scores on the six factors of the SRC model as a measure of intensity.

Table 8. Coefficients for the SRC Six-Factor Model for the 1964 Election: Cross-Pressured Individuals Only

Factor	β_i
Democratic candidate	.07432
Republican candidate	.00104
Group orientation	.08377
Domestic issues	.08121
Foreign issues	.06678
General management	.07910

N = 451
r = .64401
$\beta_0 = .49209$

Both these models are empirically plausible accounts of the voting behavior of individuals, *given the theoretical explanation of voting provided by Downs.* Because the SRC has never employed such an explanation, the tests reported in this section have not been undertaken previously. Specifically, the SRC provides no theoretical basis for the summation of factors in the six-factor model to produce an estimate of the *intensity* of the party preferences of individuals. Nor does the SRC explain the relationship between the intensity of party preferences and turnout decisions. The Downsian model, on the other hand, does both these things. Highly accurate predictions concerning both party preferences and turnout can be obtained by employing the SRC model in the context of Downs' theory of rational voting choices. We suggest that the explanation of this result is that the six attitudinal factors of the SRC model are actually surrogates for the components of the Downsian model. In other words, responses to the open-ended questions from which scores for the six factors are derived are summary comments reflecting expectations about the returns to the individual associated with the election of particular parties or candidates. If this is so, the SRC model's success in the tests described above constitutes further support for explanations of voting behavior based on the assumption of rationality.

Appendix: Operationalizing Efficacy

To relate perceived closeness of the election (variable 19) to the probability of an individual's vote making a difference in the outcome of the election, we estimated two factors. First, we developed an estimate of the probable turnout in 1964. Second, we constructed a procedure to attach numerical values to the specific responses associated with variable 19 (i.e., "win by a lot," "close race," and so on). In making these estimates, we employed the eight previous presidential elections as a base period, utilizing figures on turnout and victory margins from that period. We chose the eight elections from 1932 through 1960 because the basic party coalitions associated with the New Deal were operative over that time span. We rejected the idea of using a longer time span because we believe that fundamental shifts in the pattern of political coalitions may well affect estimations by individuals of the probable turnout in a forthcoming election. Due to the arbitrary nature of these procedures, we have also tested a variety of alternative operationalizations of efficacy.

The details of the original procedures outlined in the preceding paragraph are as follows:

1. An expected turnout of 70 million voters was estimated for the 1964 election using linear regression over the previous eight presidential elections.

2. The mean Democratic percentage of the major party vote and the variance thereof for the 1932–1960 presidential elections were treated as the parameters of a normal distribution of election results. These were then used to determine the probability of a tie vote, given the respondent's estimate of the closeness of the election, by shifting the normal distribution to a mean of 50 percent and finding the height of the curve at four points. The four points are certain arbitrarily chosen victory margins from the 1932–1960 period, selected to correspond with the response options to variable 19, as indicated in Table A–1. The weight of the normal distribution at each of the points was assumed to be the probability of a tie vote, given a probability estimate of each individual's vote for that individual's projected winner. The probability of making a difference was twice the probability of a tie, allowing for making or breaking a tie. Those who answered "don't know" or "pro-con, depends" to variable 19 or "don't know" to variable 18 (which asks for the respondent's judgment of who will win) were assigned the mean probability of making a difference. These steps are partially codified in Table A–1.

Table 9. Party Preference Predictions for the 1964 Election: Cross-Pressured Individuals Only

Predicted	Actual: Democratic	Actual: Republican
A. SRC Model (N = 451)		
Democratic	233	54
Republican	39	125
% correct party preference predictions = 79.4 $R^{*2} = .569$ $r_{pty} = .953$		
B. Downsian Model (N = 168)		
Democratic	75	21
Republican	34	38
% correct party preference predictions = 67.3 $R^{*2} = .282$ r_{pty} undefined due to small sample size		

Table A-1. Operationalizing the Probability of Making a Difference by Voting

Respondent's Answer to Variable 19 (and election year used to operationalize the response)	Operationalization (% Vote for Winner)	Probability of Making a Difference x 1 Million
Win by a lot (1956)	57.4	.0306
Win by quite a bit (1952)	55.1	.0428
Pro-con depends[a]		.0474 (weighted average of others)
Close race, fairly even (1948)	52.4	.0551
Very close race, evenly split (1960)	50.1	.0568

[a] Operationalized as the weighted average of the efficacy scores of individuals responding with definite estimates of the closeness of the election.

COMMUNICATIONS

Expenditures and Politics: Who Gets the Cookies?*

To the Editor:

R. Kenneth Godwin and W. Bruce Shepard (1976) have criticized the literature on expenditures and present instead an alternative model based upon the linkage between citizens' preferences and actual expenditures. They suggest that political factors may distort the relationship between preferences and the policy decisions made by elites. Political factors are held to be "mediating" variables, and citizen preferences (as denoted by socioeconomic variables) the major determinants of public policy. We have both an empirical model and the application of an older one as a normative guide (i.e., those policies are to be preferred which are most in accordance with citizens' preferences).

Let us begin with an examination of their thesis that citizen preferences can (should) determine expenditure levels for various policy areas. The model is offered as a description of reality based upon some normative concerns. Yet, there is precious little evidence for any of the following: (1) that citizens have opinions on the many expenditure items that come before a legislative body (cf. Converse, 1964), (2) that the elites who make these policy decisions can determine citizen preferences; or (3) that the elites *would* employ this criterion if it were available. If Godwin and Shepard want to use the correspondence between such preferences and expenditures as a normative criterion, there is less room for objection. But it seems that they believe it to have empirical applicability as well.

While political factors are considered mediating variables, citizens' preferences are posited directly to determine expenditure levels. As indicators of preferences, Godwin and Shepard employ socioeconomic variables (p. 1129). There is thus an implicit assumption that *all* individual preference orders can be "translated" into a social decision function (cf. Sen, 1970, p. 48) if confounding "political" factors are absent. This is either patently false or so laden with unstated assumptions as to be empirically useless. The difficulty stems from the treatment of socioeconomic variables as surrogates for both individual preferences (p. 1133) *and* aggregated social preferences (p. 1127). Indeed, it is not clear whether individual and collective preferences would yield the same result. We would expect that welfare expenditures would be a decreasing function of individual income. Yet, studies have indicated strong *positive* relationships between aggregate expenditures and income.

The Godwin-Shepard approach maintains that actual expenditures reflect desired expenditures *and* any confounding effects of the imperfect mechanism of politics in translating preferences into policies. This confounding effect is called "translation error." While some other students of expenditures (Fisher, 1964) also consider socioeconomic variables to be surrogates for preferences, the more typical pattern has been to treat them as measures of development, which in turn is a surrogate for: (1) a jurisdiction's ability to provide the service in question (Dye, 1966, pp. 28ff.); and (2) the need of various groups for such services (Jackman, 1975, Chs. 2–3).

Godwin and Shepard (p. 1133, n. 20, emphasis in original) attempt to get around problems such as *incorrect signs on regression* coefficients by suggesting that we employ the absolute value of differences between the desired level of a policy and the level provided so that one avoids ". . . the largely atheoretical position that political variables affect the *direction* of translation errors." But this question of efficiency of a representative system of government is based upon a normative claim that is open to challenge. Politics, one can maintain, is precisely about whether the rich get richer or the poor get richer (or both or neither), so that the direction of translation errors simply is too important to ignore. But do Godwin and Shepard dismiss the importance of directional translation error because it is theoretically unimportant or because they simply have no way of measuring it?

The statistical model proposed to measure translation error and test for the effects of mediating political factors is difficult to understand. We are given a dependent variable, Y_{ij}, the *desired* per capita expenditures for category *i* in jurisdiction *j*. Earlier, we were told (p. 1127) that socioeconomic variables would serve as surrogates for this collective demand. But, the model proposed (p. 1134) includes a set of predictors, X_{ij}, which are also socioeconomic variables. How do we distinguish between these variables? First, note that the equation includes two other predictors not previously discussed, intergovernmental transfers and debt, but this would take us into the area of whose preferences these predictors reflect. Secondly, Godwin and Shepard suggest that if their framework is correct, "... observed expenditures may be used as the dependent variable" (p. 1134). To get a measure of translation error, however, we must take the absolute value of the difference between desired and actual spending—which would, on the second criterion, always be zero. One answer implies that desired expenditures are based upon something other than citizen preferences, the other that desired patterns are always met.

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*The comments of Kathleen Peroff, James Horney, Margaret Podolak-Warren, and three anonymous referees are gratefully appreciated.

Let us assume that we can get around this problem and that we have obtained predicted values ($\hat{Y}_{ij}$) for the dependent variables. Suppose that we wanted a measure of directional translation error and then proceeded to test for the effects of political variables. Since the initial model assumed that there was no direct relationship between these variables and expenditures, the regression equation would be:

$$(Y_{ij} - \hat{Y}_{ij}) = \hat{e}_{ij} = f(PV_{ij}) + u_{ij}$$

where $\hat{e}_{ij}$ is a vector of estimated residuals from the first regression, PV_{ij} is a matrix of values on political variables affecting translation error on expenditure category *i* in jurisdiction *j*, and u_{ij} is a random disturbance term. The first regression model, however, assumed that there were no direct effects of political variables on expenditure levels. Therefore, the covariance between the estimated residuals from the first regression and *any* of the political variables is necessarily zero. Political variables thus have no effect on directional translation error. If, however, we employ $\hat{e}_{ij}$ instead of the untransformed estimated residuals, as Godwin and Shepard suggest, there will almost certainly be some nonzero correlation between translation error and political variables. Is the reason Godwin and Shepard insist upon the absolute value of the estimated residuals theoretical or a statistical artifact based upon the fact that a measure of directional translation error simply doesn't work?

Toward an Alternative Approach

The problems with the statistical model proposed by Godwin and Shepard indicate some of the difficulties in employing that framework. The normative criterion of linking policy decisions with citizens' preferences nevertheless remains very important, even if the specific model proposed is not adequate to accomplish this. But government is not a large town meeting in which individual preferences are aggregated by some mechanism into a choice for the society, however desirable that may seem. Instead, it is a battle for control over the distribution of goods and services for which citizens band together in collective action. This view of politics was most vigorously articulated by V. O. Key, Jr., whom Godwin and Shepard mis-cite in any event.[1] Politics is viewed as a continuing struggle for the division of the spoils by organized groups, to the disadvantage of the disorganized. Highly competitive *and organized* groups competing with each other determine how the budget is to be divided. But, this approach views the under- or over-supply of goods as perhaps the most critical part of the political process, in contrast to Godwin and Shepard.

Key argued (1949, p. 130): "... if there is a single grand issue it is that of public expenditure. It follows that the grand objective of the haves is obstruction, at least of the have-nots who take only a short-term view.... The have-have not match is settled in part by the fact that substantial numbers of the have-nots never get into the ring." Thus, whose preferences matter is the first critical element in constructing a more realistic approach to the study of expenditures. Once, however, there is established party organization *and* competition, there is, according to this view, some degree of accountability for the actions of the government. Elites are not viewed as reactors to public opinion. Rather, the public responds to the decisions made by the politicians. The "grand issue" of public expenditures often involves questions of *redistribution* of wealth in a society and the politican may indeed face conflicting demands from subgroups of citizens. Not only does the accountability thesis seem more fruitful, but it also provides a clearer framework for analyzing these important expenditures.

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[1] Three hypotheses on the inter-party competition hypothesis (which has been widely misunderstood; see Uslaner, 1976) are attributed to Key (Godwin and Shepard, 1976, p. 1133), and the publication date of *Southern Politics* is incorrectly stated as 1951 instead of 1949. Two of the three hypotheses cannot be found in Key's work and indeed are the words of Lockard (1959).

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TO THE EDITOR:

The model we advanced began from a concern which is shared by Eric Uslaner: that many studies of expenditures have lumped together political and socioeconomic variables in an analytic procedure guided by the statistical criterion of explained variance rather than the theoretical criterion of testing predicted structures and relationships. In our effort to provide theoretical bases for designing and interpreting analyses of the role of political variables in policy decisions, however, we chose a different path from that used in Uslaner's prior research and present comment.[1] The differences center on the use of aggregate socioeconomic indicators, on whether absolute or directional translation error is more appropriate, on the political role of the decision maker vis à vis the citizen, and on the identification of winners and losers.

Uslaner's charge that we are inconsistent in our use of socioeconomic variables is perplexing. First, we explicitly stated that only individuals have preferences; collective demand can be defined only by applying an aggregation rule to individual preferences (p. 1133). Second, we rejected the assumption that all individual preferences can be translated into a social decision function when we rejected the median preference hypothesis based upon a majoritarian aggregation rule (p. 1133, nn. 22, 23, and 25). Third, we insisted on the need to incorporate price (p. 1134 and n. 26). Transfer payments and debt must be included in the model because they affect the price of services and thereby affect the level of services citizens will prefer.

Uslaner, using the example of direct correlation between a jurisdiction's aggregate wealth and welfare expenditures, suggests that our use of socioeconomic variables is inconsistent with previous studies where these variables were used as measures of development. Development was used either as an indicator of a jurisdiction's potential to provide the service (Dye) or as an indicator of the needs of groups for such services (Jackman). Actually, our model integrates the two interpretations while avoiding such imprecise terms as "development" and "need." We chose the language of supply, demand, and price. Aggregate wealth may indicate a jurisdiction's potential to provide a service; but individuals must pay taxes if that ability is to be actualized. Presumably in a wealthier jurisdiction individuals are willing to purchase more services because the price or tax share is a monotonically decreasing function of aggregate wealth. The "group need" hypothesis may be reformulated as follows: the demand for a service is a function of the number of individuals whose socioeconomic circumstances lead them to desire the service. Whether the language is "ability" and "need" or "price" and "demand," however, the correlation between expenditures and community wealth indicates linkage. Our model is designed to discover what kind of linkage this is.

Uslaner disregards our theoretical reasons for using absolute rather than directional translation error and alleges that we use absolute values to "get around" problems of incorrect signs and to take advantage of a statistical artifact. The first charge makes no sense. The subsequent use of the absolute value in no way affects whether the signs of the regression coefficients of the demand equation are found to contradict one's expectations. Second, for linkage models to fit, "some correlation" is not sufficient; there must be correlation of the predicted shape and direction with specified variables (pp. 1131–33).

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[1] Eric M. Uslaner and Ronald E. Weber, "The 'Politics' of Redistribution: Toward a Model of the Policy-Making Process in the American States," *American Politics Quarterly,* 3 (April 1975), 130–70. We also wish to acknowledge our gratitude to Professor Uslaner for providing us with a draft of his comment at the same time he submitted it to the *Review.*

A central difference between our model and Uslaner's alternative concerns the role of the political decision maker. Uslaner misrepresents the thrust of our approach by indicating that our model views political factors as "distorting" or "confounding" the translation of preferences into policy. He chooses to ignore that the model, grounded as it is in linkage theory, explicitly argues that the appropriate role of political variables is to "link the demands of citizens . . . to the policies" (pp. 1129–30). We postulate that citizens have preferences and that political elites attempt to translate these preferences into policy. Our model, as Uslaner indicates, is both normative and empirical. It identifies a goal (accurate translation) and offers predictions concerning which political institutions will better approach this goal under varying assumptions.

Uslaner argues for a quite different view of political decision makers. In the final paragraph of his communication he writes, "Elites are not viewed as reactors to public opinion. Rather the public responds to the decisions made by the politicians." This view is consistent with the second paragraph of his communication where he suggests that citizens probably do not have opinions on expenditure items; and that even if they did, elites either could not or would not use them. These comments are, however, inconsistent with Uslaner's alternative model where he argues that "whose preferences matter" is "the critical element" in the struggle over the budget and that "party organization *and* competition allow accountability for the actions of decision makers." Presumably if citizens have no opinions or if elites do not respond to the opinions the citizens do hold, there would be neither struggle nor accountability. This inconsistency is made still more confusing by the fact that Uslaner's "alternative" is actually the political parties portion of our model (p. 1129) except for his use of directional translation error.

Fortunately for our model, and Uslaner's, considerable evidence supports our assumptions concerning citizen opinions and the role of decision makers. Using improved methodologies, recent surveys find citizen expenditure preferences to be highly reliable and valid.[2] Studies of concurrence between elites and citizens support both the empirical validity of our assumptions and the predicted relationships between elites and citizens—i.e., concurrence significantly increases under conditions of community homogeneity, increased citizen participation, greater party competition, and the presence of unreformed governments.[3]

The final issue in Uslaner's comment concerns whether absolute or directional translation error is preferable. Uslaner suggests that our mode of analysis ignores who are the winners and losers in politics. Must one give up that crucial question to pursue the theoretical issues for which our model of analysis was proposed? We think not. Research using concurrence scores—the analog of translation error for survey data—can and does investigate concurrence with whom.[4] Yet, Uslaner is right in pointing out that the question was ignored in our original article. In writing the article we felt (and still feel) that before predicting winners and losers and explaining distributional outcomes, logically one must know which of the many forms of linkage (or what we called "nonlinkage") are actually occurring.

In more recent work, we refine the concept of translation error to recognize "whose preferences matter."[5] Briefly stated, we adapt the median preference aggregation rule by assuming that the regression equation predicts the median expenditure preferences for a set of politically influential citizens. Using this simple, even tautological assumption, we make different deductions about the relationship between translation error and factors facilitating linkage. These deductions bring the issue of whose preferences are (and are not!) linked to policy outputs within the scope of our model and allow its empirical examination.

Admittedly, our framework requires a major change in customary approaches to the study of policy determinants. To continue to utilize directional translation error would mean the continuation of the largely atheoretical position which our article challenged. Uslaner has elsewhere called for such a major change: "It is time to formulate *linkage* or process models of the policy-making process, even at the risk of proposing hypotheses which might prove untenable."[6] We took that risk. Increasing empirical evidence indicates that our model is not only tenable but is strongly supported.[7]

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[2] Philip Beardsley, David Kovenock, and William C. Reynolds, *Measuring Public Opinion on National Priorities* (Beverly Hills, Calif.: Sage, 1974).

[3] In addition to the studies cited in the original article more recent evidence is found in Susan B. Hansen, "Participation, Political Structure, and Concurrence," *American Political Science Review*, 69 (December 1976), 1181–99; Susan Hansen, "Linkage Models, Issues, and Community Politics," Department of Political Science, University of Illinois, Champaign-Urbana, mimeographed; and Paul D. Schumaker and Russell W. Getter, "Responsiveness Bias in 51 American Communities," *American Journal of Political Science*, 21 (May 1977), 247–81.

[4] Sidney Verba and Norman Nie, *Participation in America* (New York: Harper and Row, 1972), pp. 299–333.

[5] "Public Policies and Political Linkage: A Refinement and Test of Translation Error" (paper presented at the annual meeting of the Western Social Science Association, Denver, April 1977).

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The Minimum Winning Coalition

TO THE EDITOR:

William Riker's "size principle" introduced in the *Theory of Political Coalitions* indicates that rational individuals, when faced with the necessity of forming coalitions to affect collective action, will "create coalitions just as large as they believe will insure winning and no larger" (Riker, 1962, p. 4).

Russell Hardin has suggested (1976) that the size principle "does not seem to apply" to House of Representative roll-call data for eight Congresses. He indicates that "roll-call voting in the United States House of Representatives overwhelmingly violates the size principle" (pp. 1213–14). Hardin's generous conclusion is that "game theoretic coalition theory adds little to our ability to explain legislative coalition formation."

To prove his point, Hardin has summarized some congressional data previously used by David H. Koehler (1972, pp. 149–64) which showed that 52 percent of winning coalitions contained less than 65 percent of those representatives voting. Hardin points out that in a 401-member random legislature run by flipping coins, 99.9 percent of all winning coalitions would contain less than 65 percent of the membership. Since the empirical voting record of the House of Representatives does not seem to meet the outcome predicted by Hardin's random legislature test, he concludes that "the conditions for the size principle are not met in the House of Representatives" (p. 1213).

Actually, the random legislature model used by Hardin is not an appropriate theoretical representation of the data he presents for the House of Representatives. First, all members of the House of Representatives do not vote in each of the roll calls; Hardin, however, assumes that each member votes on each issue. Using the data for the eight Congresses, one finds that members voted only about 85 percent of the time in roll-call votes. Secondly, the correct way to summarize the median winning coalition is in terms of the members both voting and nonvoting; not by tabulating "observed coalitions" which take into account only voting members.

Given these two important points, the data which Hardin analyzed may suggest a very different interpretation. Consider for a moment that over the eight Congresses, 85 percent of the time members chose to take part in a given roll-call vote. Of those members who did take part in the roll-call votes, 61.9 percent of them voted "yes" and 38 percent of them voted "no." Keep in mind, however, that 14.7 percent of the members of the House did not vote. Now if these nonvoting members of the House mirrored the population of those members who had voted, Hardin's analysis might indeed be correct. Hardin implicitly makes the assumption that the nonvoting members of the House are distributed probabilistically in exactly the same fashion as those members of the House who did vote.

Can it be so that the nonvoting members are distributed in the same way between "yes" and "no" votes as those members of the House who did vote? Of course not; this implicit assumption in Hardin's analysis is clearly not the case. In order to keep members of the winning coalition in line, the manager of the vote must insure that each of the members who will vote on a particular measure does indeed cast a ballot and is visible to all other members of the coalition as doing so. The manager will make very clear to those members in the coalition that the benefits of logrolling will adhere only to those members actually voting on the particular measure. This would mean that we would expect in any given roll-call vote that all members of the winning coalition for a particular measure would indeed vote. What about the motivation, however, of those individuals, who, if they voted, would vote against the measure? Is there an incentive for them to actually cast a ballot? The answer is perhaps not. If the coalition has been carefully formed, even considering some imperfect knowledge among members of the House, those opposing a particular measure would have no peculiar incentive to be counted among the opposition.

In fact, those opposing a particular measure may reasonably not vote, explaining to their constituents that to cast an opposing vote might cost the constituents support for their own favored measures.

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[6] Uslaner and Weber, "The 'Politics' of Redistribution," p. 162. Italics added.

[7] See note 3.

Take the very strong assumption, then, that all of those members who did not vote would have voted "no" had they actually voted. If this were the case, and once again using Koehler's data, the actual median winning coalition of 230 members is only 52.8 percent of the total number of members in the House. Since this number is just slightly more than 50 percent, it appears to follow much more closely Riker's size principle. Viewed in this manner, there does not seem to be the magnitude of agreement on roll-call votes in the United States Congress which Hardin surmised from the data.

To take a theoretical situation closer to that described by Hardin, consider the following random legislative model depicted in Figure 1. Assume a 401-member legislature in which 201 members have already formed a coalition on some issue.

Recall that the manager of the vote will see that each coalition member actually casts a ballot. The remaining 200 members of the legislature must decide whether they will vote or abstain. Suppose the overall percentage of members voting is 85 percent. The probability that one of the 200 will vote is then

$$\frac{401 \times .85 - 201}{200} = 69.93\%$$

Furthermore, suppose that the probability is ½ that any one of the 200 will vote "yes" should he or she decide to vote. Examining Figure 1, the observed or reported percentage of positive vote would be

$$y = \frac{x + 201}{z + 201}$$

Using this model, it is possible to generate the frequency of observed coalitions in Table 1. This table is computed assuming P = ½ (that is, the probability of a "yes" vote should the legislator vote). Now consider what happens when the value of P moves over the range .1 to .9. Table 2 indicates that the percentile containing the *observed coalition* will steadily move upward as P increases. One can clearly generate *observed coalitions* similar to those calculated by Hardin which do not in the least challenge minimum winning coalition theory. Note that the reason for our disagreement with Hardin's findings is the result of perceiving only *observed coalitions* (i.e., the number of voters voting for a winning measure divided by total members voting). In Hardin's case the number of members voting is always 401 while in our case there is only a probability that a legislator will vote. Hardin's theoretical model is only the special case where the overall percentage of members voting is 100 percent *and* the P value

Table 1. Size of Observed Majority Given P = ½

Size of Majority (%)	50–55	55–60	60–65	65–70	70–75	75–80	80–85	85–90	90–95
Frequency of occurrence	–	–	–	–	.00685	.60133	.38953	.00125	–

Table 2. Size of Observed Majority Given Varying P

P	.10	.20	.30	.40	.50	.60	.70	.80	.90
Percentile containing observed majority	60–65	65–70	70–75	75–80	75–80	80–85	85–90	90–95	95–100

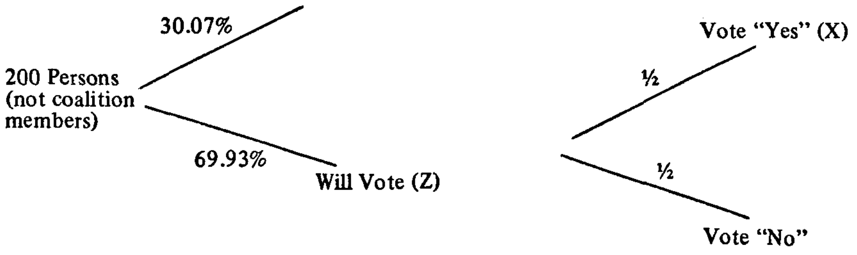

Figure 1

is very low. There is little reason to believe the actual *observed coalitions* would closely match the predictions of such a model. Our thanks to Gordon Tullock who originally suggested that we examine this situation.

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BARRY KEATING
KEITH LEE

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TO THE EDITOR:

Barry Keating's and Keith Lee's speculations about what might take place in the House of Representatives do not provide creditable arguments for the size principle's applicability to House roll-call voting behavior. In essence they present two defenses of the size principle. For the first they rely on what they acknowledge is a "very strong assumption," so strong indeed that it is counterfactual, as a few minutes inspection of *Congressional Quarterly* roll-call data would have revealed to them.[1] For the second they simply assume the size principle's validity and then wave away all votes with the winners in excess of what would be needed if all members of the House simultaneously turned up for a vote. I will address their first defense at some length and then their second defense briefly.

Keating and Lee question whether nonvoters in the House divide themselves over an issue in the same way voters do, noting that in a test of the size principle against House roll calls I implicitly assumed they do. They counter my assumption, saying it "is clearly not the case." To prove their point, they present an argument. That is odd, since the epistemology of positive theorists requires that they settle questions of what is the case by looking at facts rather than by turning introspectively to their intuitions. For a moment, before facts clutter the page, consider their argument a priori.

Keating and Lee claim that representatives on the winning side of an issue have greater incentive to vote than those on the losing side, so great an incentive that they will all almost certainly vote. Hence, virtually all nonvoters can be counted with the losers. Adding all non-voters to the losers yields a median winning coalition of 52.8 percent on several years of "contested" roll-call votes, i.e., those with majorities less than 85 percent. This size is not far from minimum winning when all representatives vote. Can Keating and Lee take this argument seriously a priori? Surely not. By their addition, nearly half the winning coalitions are outnumbered by the losers. Since the latter had the votes, why didn't they win? From a third to half of the time in any year, the losers evidently lose because they are lazy; most of the rest of the time, of course, they lose because they get overwhelmed in gross defiance of the size principle.

Now consider the messy facts, a rabble who delight in tearing apart singular explanations. *Congressional Quarterly* tabulations of House voting participation include four categories of non-voters: those officially paired for or against (on average 2 percent on each roll call), those who have publicly declared their positions, those who respond to *Congressional Quarterly* polls saying how they would have voted, and those whose positions cannot be ascertained because they do not put themselves on the record in any of these ways or by voting. Half of the nonvoters put their positions on the record. My casual survey of many roll-call votes in various recent years suggests that nonvoters who put themselves on the record sided with the winning coalitions somewhat more often than representatives who voted.

How about the remaining 5–10 percent of the membership, those who are not on the record? They are disproportionately drawn from a small number of chronic offenders, including career-long nonvoters (Edward Hebert, capping his career, was off the record 51 percent of the time in 1976). Many representatives begin to neglect congressional affairs because their life fortunes are in decline (Andrew Hinshaw was off the record 100 percent of the time in 1976, Wayne Hays 44 percent, and Allan Howe 31 percent). Some grow tired at the ends of their careers (Joseph Karth was off the record 65 percent in 1976 and Edwin Eshelman 36 percent). Some are ill (William Barrett was off the record 69 percent of the time before he died in 1976, Torbert Macdonald 67 percent, and Wright Patman 53 percent). In election years many forget the House while running, often in vain, for higher office (Sam Steiger was off the record 63 percent of 1976, Alan Steelman 59 percent, Marvin Esch 59 percent, Donald Riegle 59 percent, Jerry Litton 53 percent, William Green 51 percent, John Conlan 51 percent, Alphonzo Bell 49 percent, Peter Peyser 49 percent, and William Stanton 48 percent, all while running for the Senate). The few representatives cited parenthetically in this paragraph (4 percent of the membership) account for a third of all off-the-record nonvoting in 1976. Much of the remaining two-thirds occurs in conspicuous chunks rather than sporadically, which strongly suggests that neglectful members are not being selective. Consider a few unsystematically chosen examples in 1976: but for occasional pairings, Spark Matsunaga was off the record for over a month and 148 consecutive votes of the 661 House roll calls; Dale Milford was off for 108 consecutive votes; Patsy Mink 95 and 36; John McCollister 87; Robert Giaimo 67; James O'Hara 59; George Hansen 57; Ray Roberts 56; Claude Pepper 50; and John Flynt 48.)

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[1] House roll call data from *CQ Almanac* (Washington: Congressional Quarterly, various years, 1969 through 1976).

In sum, half the nonvoters are on the record with divisions much like those of the voters, and about half the remainder are off the record in such wholesale fashion that their abstinence probably approaches a random distribution among winners and losers. The remaining 3–4 percent of off-the-record nonvoting might result from implausibly one-sided incentives such as Keating and Lee suggest, but even then it would not substantially affect the observed distribution of winning coalitions by size to add all 3–4 percent to the losers or the winners. Hence, the implicit assumption that nonvoters are like voters in House roll calls is relatively accurate.

Turn now to the second defense. Keating's and Lee's model of House voting presented in Figure 1 and Tables 1 and 2 cannot be taken seriously. They conclude from it that observers see large voting majorities and erroneously conclude that the winning coalitions are too large for Riker's size principle. In their model, a precisely minimum winning coalition (a constitutional majority) forms and all its members show up to vote their victory. (Again, of course, this factual assumption is grossly violated, since a third to half of the winning majorities of Koehler's "contested" votes are smaller than the constitutional majority size.) Other members of the House show up randomly and then vote randomly, many of them perhaps voting with the winners, thereby deceiving atheoretical minds into thinking the number of winners is more than minimal. Keating and Lee are not deceived—they know these overlarge observed coalitions "do not in the least challenge minimum winning coalition theory." What would? Nothing which could actually be observed, it seems.

Keating and Lee generally object to findings based on a study of observed coalitions. But note that looking at observed (i.e., recorded) votes is far more likely to have an effect opposite to that of Keating's and Lee's speculations. There have been historically several times more unrecorded teller and voice votes than recorded votes in the House. Many of the unrecorded votes are on more important issues than many of the issues decided on the record. But these unrecorded votes presumably include disproportionately many large majorities since closer divisions could be forced to recorded votes by the losers. Again, the conspicuous fact is that voting coalitions in the House only infrequently approach the minimal winning size. Anyone who attempts to fit a theory of winning coalition size to the data on Congress (and other American legislative bodies) will therefore know why Heinz Eulau (1967, pp. 26–54) sought "logics of rationality in unanimous decision-making."

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Reference: Heinz Eulau (1967). "Logics of Rationality in Unanimous Decision-Making," in Carl J. Friedrich (ed.), *Rational Decision.* New York: Atherton Press.

On Theories of Development and Dependence

TO THE EDITOR:

In his review (*APSR,* 70 [March 1976]) of Dr. Brett's *Colonialism and Underdevelopment in East Africa,* Dr. Berman writes that the book "began as a doctoral thesis written in the empiricist and largely uncritical ethos of the Institute of Commonwealth Studies of the University of London." That sounds bad for the Institute. However, "Cet animal est méchant: quand on l'attaque, il se défend." I should like to offer a couple of comments on the matter.

There is, for one thing, an initial ambiguity. It is not made clear whether the judgment quoted above is Berman's own or that of Brett. In fact, it is not a direct quotation from Dr.

Brett's own preface, though there is such a quote later in the same paragraph, but instead a paraphrase of the following from Brett: "In London I worked in the general ethos of the Institute of Commonwealth Studies where the tendency in colonial history was towards a thorough empiricism in a framework which did not question the overall validity of Britain's colonial contribution to African development except where extensive settlement had been allowed." We note that "empiricism" is common to both, but the meaning of "largely uncritical" is illuminated by Brett's more precise phrasing.

"Empiricism" used to be the name given to a general philosophical theory of knowledge. But Brett-Berman appear to intend it as a derogatory term to denote mindless fact-collecting conducted without the guidance of any general theory. This is nowadays quite fashionable usage on the part of Marxist writers; indeed it is more useful as a badge of their affiliation than as a reliable account of the position of their opponents. With respect to the Institute at the period in question it is misleading in two ways. In the first place, the Institute's primary role was to serve as a forum for scholarly discussion. I very much doubt if it could be shown that there was any doctrinal bias in the choice of papers for presentation or in the composition of those attending the seminars. All views seeking expression were given to a hearing. Whether the hearing was sympathetic certainly depended on the extent to which the interpretations offered were supported by adequate hard evidence. But that, I take it, is a mark of critical, not "uncritical," historical scholarship. (In the same paragraph Berman contrasts the "ahistorical character" of "much previous research" with the "more historically rooted global perspective" of underdevelopment theories. It is difficult to understand this "more historical than thou" claim, except as a mere assertion that the only true history is one shaped by a given doctrine.) Second, the suggestion that there was a guiding spirit which was hostile to "theory" as such is not persuasive. In so far as a "tone" may have been set by my predecessors, Sir Keith Hancock (1948–1957) and Dr. Kenneth Robinson (1957–1966)—Brett does speak of the "dominance of the colonial history tradition *à la* Hancock"—it would not fit this simple picture. Both would have respect for evidence, but both would be concerned with its valid interpretation. Hancock, an economic historian, might be drawn toward more general explanations, Robinson remaining skeptically aware of variations in time and place which would permit much less general interpretations.

But there is a further point which has to do with dates. As Brett says in his preface, dated 1972, the work on the book had been with him for "more than ten years." The research was completed in 1965, the doctoral thesis presented in 1966. Now Berman himself writing in 1975 does not claim more than that it is "during the past six or seven years" that "established theories" of development have "increasingly been abandoned" (by implication in favor of underdevelopment theories). Surely it is evident that between 1965 and 1975 not only Brett but the nature of the intellectual debate on colonialism changed. In that case, Brett-Berman may look back on the unregenerate early sixties with some disdain, but they should not speak as if scholarly institutions were at that time blocking the fresh winds of truth—when in fact such winds were simply not blowing. Who is being "ahistorical" now? Equally, Berman should not make a statement which can be interpreted as implying that the Institute has since those years remained indifferent to the changes in the discourse on colonialism and neocolonialism. The Institute's "ethos" has indeed not changed, but precisely because it continues to attach importance to its role as a forum, it has entertained much discussion on the new theories. Speaking for myself alone, I find that while such theories direct attention to some important kinds of evidence and give them a coherence, they still offer only a partial account of situations too complex for single-doctrine explanation. Only for those who are certain of holding the whole truth would this be a declaration of dedicated hostility. I trust that we shall continue to seek explanations but also to require that they be subjected to criticism in the light of evidence.

Finally, I am prompted to a more speculative point about values. Scholars must try to be coolly analytical, but they usually find it a hard job because they have feelings and preferences like everyone else. In no area is this more obvious than in work on the new states. Berman, correctly I believe, suggests that it is the "failures of the so-called developing nations" which have stimulated the new underdevelopment theories. He also says that these theories are "pessimistic about the impact of linkages between developed capitalist states and the Third World." By contrast his straw man—empiricist, ahistorical and uncritical—is a man of "sanguine optimism." There is one important sense in which all this is true. Those who were drawn to the study of new states were probably more moved than they recognized by ties of affection with subject peoples and idealistic hopes for brave new worlds. When the postcolonial dawn proved, sometimes very promptly, to be more than a little murky, the sense of dismay was correspondingly profound. Independence was then "the god that failed." In that sense, the optimism of the early sixties has been difficult to sustain. Most of us are much sadder now, striving to get wiser by a re-examination of previously held positions, daring to hope for no more than patchy, piecemeal improvements. Today's underdevelopment theorists may well be those whose idealism and indeed missionary spirit toward the Third World was greatest, their sense of betrayal all the more total. To account for the god that failed, they have to find a devil at work; the false dawn past, they look to a new one. They are almost (but not quite) to be envied for being able once more to combine brightest hope with darkest anger.

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TO THE EDITOR:

Professor Morris-Jones's letter raises several issues of substance concerning the development of theories of dependence and underdevelopment. These can only be dealt with briefly and superficially in this context, but I feel they do require further clarification.

Let me say first, however, that I fully accept Professor Morris-Jones's correction of my all too casual and misleading paraphrase of Brett's remarks about the Institute of Commonwealth Studies. As an occasional visitor to the Institute's seminars in the early 1960s when I was a graduate student at LSE and later in 1968 when I pursued part of my doctoral research there, I know from personal experience the wide range and high standards of scholarship it has sustained over the years. Nevertheless, the more general problem remains of the tendency of the Anglo-American scholarly community to avoid questioning, before the last decade, the character of colonial rule, the nature of and reasons for decolonization, and the concept of "development" itself: as well as accept as a given the existing structure of world capitalism and the essentially benign motivation and effects of Western economic and political involvement in the Third World. This is something of which most of us were guilty, not just the Institute of Commonwealth Studies.

Empiricism in this context involved not so much a methodological doctrine as the practice of research. Social science research on underdeveloped societies became progressively more specialized and fragmented in the analysis of specific and partial issues, with decreasing reference to more general theoretical and methodological problems. By the mid-1960s an academic division of labor had appeared within which a large number of scholars dealt with the specifics of field research in the Third World, leaving the larger issues of theory and synthesis in the hands of a smaller number who built abstract models and typologies on the foundation of the unquestioned assumptions. (The work of the Committee on Comparative Politics of the Social Science Research Council, as well as the individual work of such scholars as Apter, Deutsch, Pye, Huntington, Almond, and Powell comes to mind immediately in this context.) The "nature of the intellectual debate on colonialism" and on the origins and meaning of "development" and "underdevelopment," as well as on several other basic questions has indeed changed over the past decade, as the work of such scholars as Andre Gunder Frank, Fernando Cardoso, Hamza Alavi, Samir Amin, Mahmood Mamdani, Colin Leys, and Edward Brett himself, to name just a few, has increasingly revealed that the elaborate edifice of "development" theory was built on quicksand. Under the circumstances it is not surprising that some have abandoned the structure of liberal development theory and sought elsewhere firmer ground on which to build. Others, caught high on the swaying structure, have clung even more tenaciously to it. (See for example, Leonard Binder et al., *Crises and Sequences in Political Development,* or Gabriel Almond et al., *Crisis, Choice and Change.*)

On the issue of the "ahistorical character" of much previous development theory and research, Professor Morris-Jones's protest is legitimate to the extent that this failing is more characteristic of American than of British scholarship, which largely retained a historical consciousness of at least Britain's imperial experience and maintained a degree of skepticism about the abstract theories emanating from North America. I hope he would agree that we have had enough studies of underdevelopment in Third World societies, ranging from older works by McClelland, Hagen, and Lerner, to such recent examples as Michael Lofchie, ed., *The State of the Nations,* or the *Crises and Sequences* volume noted earlier, that variously employ the mystified categories of "traditional" and "modern" society, profess to analyze and measure the factors leading to "modernization," or locate the problems of underdevelopment in the "values" of "traditional" society with scarcely a mention of colonialism and imperialism and their profound impact on the historical forms of social structure and culture. The real issue, of course, is not that "the only true history is one shaped by a given doctrine," but that historical understanding does not exist outside of a general theoretical framework, whatever it happens to be. Even to ignore history, as is characteristic of "cross-national aggregate data analysis" of development, or to treat it as if it began at the moment of national independence reflects a methodological or ideological position, whether a particular writer is aware of it or not.

As to the history of the emergence of various theories of underdevelopment, dependence, neocolonialism, etc., I should point out that my original review was written in mid-1974 and I was specifically thinking of the 1966–67 period in the U.S., during which the appearance of the work of Andre Gunder Frank is often credited with marking the beginnings of the contemporary debate on development theory. The roots of this approach can, of course, be traced to the work since the late 1940s of non-Marxist Latin American structuralists such as Raul Prebish, Osvaldo Sunkel, Celso Furtado, and Pablo Gonzalez-Casanova; or to the beginnings of this century in the classical Marxist writings on imperialism by Lenin, Hilferding, Bukharin, and Luxemburg. However, I think we should recognize the general influence of the Cold War era and of doctrinaire anticommunism in narrowing the scope of debate on development and largely excluding Marxist approaches from serious consideration. Thus even so important a work as Paul Baran's pathbreaking *The Political Economy of Growth* was long ignored by liberal academic social science. As a graduate student in the U.S., I can remember first hearing of it only in 1967, reading it with the assumptions of liberal developmentalism, and first rejecting it as a polemical tract.

For many American scholars the Vietnam war must have been a crucial watershed in opening to question the entire range of U.S. involvements in the Third World. I know that for myself and many of my peers in graduate school and junior staff positions in the late 1960s and early 1970s our growing opposition to the war was often the start of a more profound and difficult shift in intellectual orientation rooted in an increasing discontent with the scientific inadequacy as well as unacknowledged ideological preconceptions of liberal development theory. For others, direct witness to and analysis of the consequences of U.S. imperialism in other parts of Asia or in Latin America may have been the trigger for the development of a more critical perspective. Certainly the painful and often bitter clashes which erupted during this period in the area studies associations specializing in Asia, Latin America, or Africa, are symptomatic of these processes of intellectual and political change.

For this reason Professor Morris-Jones's characterization of contemporary Marxist social science in association with such terms as "the god that failed," etc. misconstrues the origins of contemporary analyses of imperialism and underdevelopment and suggests misleading analogies to the rigidly doctrinaire and party-focused Marxism of the 1930s and 1940s. Many radical development theorists, such as Richard Barnet and Ronald Muller, Immanuel Wallerstein and Richard Sklar, and even Frank himself, are not Marxists and would probably resist such a characterization of their work. Furthermore, I think it is simply impossible to dismiss contemporary western Marxism as a "single doctrine." Such modern writers as James O'Connor, Ralph Miliband, Lucio Colletti, Perry Anderson, Ernst Mandel, Nicos Poulantzas, Jurgen Habermas, Claude Meillasoux, and Harry Braverman represent an incredible diversity of approaches marked as much by disagreement as consensus. This diversity illustrates the growth of a vital Marxist scholarship beyond the bounds of party doctrines generally and the emancipation of Marxist theory from its assumed association with the dead hand of Soviet theoretical orthodoxy in particular. Within all of the social science disciplines Marxism has emerged with an increasingly credible claim to be the creative and critical cutting edge of intellectual development—a challenge that liberal social science cannot casually dismiss.

The varied approaches to Marxist theory that have developed do share, it seems to me, at least two basic features that distinguish them from liberal social science. While many of the perspectives of underdevelopment and dependency can be appropriated by a liberal revisionist approach to development (see, e.g., Hollis Chenery, *Redistribution With Growth*), Marxist analysts have been far more forthright in seeing that a critique of underdevelopment at the periphery leads back to and is incomplete without a critique of capitalism at its core in advanced metropolitan states. As Lukács noted long ago, it is this "point of view of totality" that is one of the unifying features and greatest strengths of Marxist analysis. This perspective also recognizes that valuation and normative commitment are an intrinsic and inescapable part of social analysis. Moroever, they constitute no necessary barrier to objectivity, but are in fact both necessary and desirable in any social science worthy of the name.

I agree with Professor Morris-Jones that most students of the Third World are probably sadder men and women today than they were ten or fifteen years ago. Critical reflection, however, might lead us to conclude that many of the idealistic hopes of the past were unfounded to begin with. Certainly those who began from a Marxist position, such as Baran, entertained no such expectations. I must therefore emphatically disagree with Morris-Jones's conclusion that we can hope for "no more than patchy, piecemeal improvements." Such a perspective suggests that while "previously held positions" may be questioned and even discarded, they are being replaced with a passive acceptance of the fundamental immutability of the structural status quo that begs the basic theoretical and human issues. Moreover, it is a position that is challenged not only in theory, but also in the practice of the few revolutionary governments in the Third World—e.g., in Cuba, China, Mozambique, and Vietnam, which alone have begun to make significant progress towards the transformation of the conditions of underdevelopment and imperialism. It is not the devil at work in the world, but real historical forces which need to be understood and acted upon, and to this end a Marxism of "brightest hope and darkest anger" holds greater promise of success than a liberalism of despair and resignation.

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Observations of a Gentle Reader

TO THE EDITOR:

For some time I have intended to express my views concerning the editorial comment published in the December 1976 issue of the *American Political Science Review* (p. 1030), which I gather sets the editorial guidelines for the future.

The distinction between the "gentle citizen" or the "gentle reader," on the one hand, and professional political scientists, on the other, constituting a community of "co-investigators" who publish in the *Review* findings which they alone can understand and share, is ingenious. But you need to define the nature of the subject matter and the nature of the common sharing. You define it to be what you want it to be and, in so doing, define your readers as well. Thus political scientists are defined to be those who read the *American Political Science Review* and share the common knowledge, methods, techniques and theories as they cumulatively unfold themselves in the pages of the *Review*.

I really have no quarrel with such a definition. But I fear that it defines out of the community of political scientists the majority of those who call themselves political scientists, who are members of the Association, who hold Ph.D.s in Political Science and who teach in reputable political science departments. It relegates them to the not so unpleasant limbo of "gentle readers." This is my difficulty. The *Review* belongs to the political scientists and the editor's freedom to define them must, in the last analysis, take into some consideration their freedom to define themselves, the *Review*, and, if need be, its editor.

The articles that have been appearing in the *Review* over the last three or four years constitute, aside from some notable exceptions that make the rule, exercises in quantitative applications of mathematical and statistical techniques (that most of us do not have) to analyze irrelevant behavioral manifestations and to reach spurious correlations (in which most of us are not interested). I have been asked quite a few times to evaluate some manuscripts and my response is invariably the same: "the substantive material they deal with (about which I know something) is spotty and unreliable but the techniques used to analyze it show a high level of mathematical sophistication (about which I know nothing)." Yet, whenever I showed these manuscripts to colleagues in mathematics or statistics, they gave me exactly the reverse evaluation!

I find myself increasingly unable to read the *Review* and I would have long ago relegated myself to the dust-bin of history or to the ranks of the "gentle readers" had I not discovered that most of my colleagues shared the same feeling of frustration and perhaps anger. Somebody reminded me that of the last 20 presidents or so of the Association, the greater number belonged to the group of "gentle readers" and that some had their books endorsed even by that very citadel of gentility, the Book-of-the-Month Club.

It may well be that I and many others are wrong. There is one test: why don't you ask your readers, before you define them? Survey work, despite its limitations, is occasionally useful and this may well be a case.

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EDITORIAL NOTE

Space limitations dictate restrictions on the length of "Communications." Submissions must not exceed five double-spaced pages, including references, footnotes, and tables. All but the briefest letters will be refereed.

The March issue sports a new cover. The greater number of articles published in recent years has made it impractical to place the full table of contents on the cover. During his editorship, Nelson W. Polsby listed all names but only a few articles on the cover. We have now removed them all. We have also broken out in new colors. The border will change color with the seasons—green for March, red for June, gold for September, and white for December. Hopefully placing that bit of color on the spine too will help our readers pull the correct number off the shelf, and thus encourage readership. Perhaps the most radical change of all is the use of arabic numbers for the volume (an Edward R. Tufte suggestion).

Finally, you will observe below that we are now listing only those manuscripts accepted since publication of the last number (a Paul Abramson suggestion).

Articles Accepted Since Last Listing

Steven J. Brams, New York University, and Peter C. Fishburn, Pennsylvania State University, "Approval Voting"

Robert K. Faulkner, Boston College, "Bickel's Constitution: The Problem of Moderate Liberalism"

Gary C. Jacobson, Trinity College, "The Effects of Campaign Spending in Congressional Elections"

Lance T. LeLoup, University of Missouri-St. Louis, "Reassessing the Mediating Impact of Legislative Capability"

S. Sidney Ulmer, University of Kentucky, "Selecting Cases for Supreme Court Review: An Underdog Model"

Erratum

In "Power Consciousness: A Comparative Analysis" by Joel D. Aberbach (December, 1977), p. 1551, second column, brackets were erroneously inserted in the last line of quoted survey responses. The final sentence should read, "There are too many to actually know what each other are doing," a direct quote.

BOOK REVIEWS

Political Theory and Methodology

The Shotgun Behind the Door: Liberalism and the Problem of Political Obligation. By Philip Abbott. (Athens, Georgia: University of Georgia Press, 1976. Pp. xv + 206. $8.00.)

This book is a suggestive contribution to the considerable recent literature concerning issues that nest under the rubric of political obligation. It treats these issues in the setting of liberal theory. Liberals have attempted to combine a moral outlook emphasizing autonomous judgment with the notion of obligations to obey the state, an institution that is "by nature evil." This attempt has failed and liberals have sacrificed their moral views "to the requirements of political order." More important (especially since "liberalism" is used without much historical discrimination), the attempt *must* fail. "A serious attention to moral judgment forbids a moral theory of political obligation." The author accepts "the initial premises of liberal ethical theory . . . while discarding the theories of political obligation that have been attached to it." This effort yields, most notably, (1) a rehabilitation of the notion of conscience as a necessary feature of morality; (2) a recommendation to scrap the concept of political obligation as having "no place in a liberal society"; and (3) an argument that "trust" provides us "with the only accommodation which the fundamental irreconcilability between moral judgment and . . . political authority allows." This reviewer finds the arguments for these contentions unpersuasive, but they certainly deserve attention.

"Conscience" and "conscience talk" do not involve a private faculty of moral knowing, a hostility to moral rules or principles, or a self-righteous moral posture. Their role in our discourse is to emphasize a defining dimension of the *process* of moral judgment. A judgment is moral only if it is made autonomously, "by reference to standards of action which [the agent] . . . has chosen." Expressions such as "it's a question of conscience" and "I couldn't with good conscience do that" are merely ways of saying that the issue at hand is a moral one that agents must decide for themselves rather than on the basis of any species of external influence or authority. The agent may discuss and consult, consider established rules and principles, and must defend the judgment to others. But the decision must be made autonomously.

It is by no means clear that "conscience talk" actually works in the innocuous manner Abbott suggests. What is more important in terms of his larger argument is that his sparse account of autonomous judgment, which wrestles almost not at all with the harder questions about what this is to be distinguished from (for example those posed by neo-naturalists such as Phillipa Foot) will not yield the conclusions he wants to derive from it. In particular, it will not yield the conclusion that there is a necessary incompatibility between moral judgment and a moral theory of political obligation. The latter, after all, is an attempt to give reasons for deciding certain recurrent kinds of questions in particular ways. Nothing in Abbott's purely processual account of autonomous judgment could impede conscientious acceptance of such reasons.

Abbott's thoughtful critique of consent, contract, and benefits-received theories may point toward a more substantive concept of autonomy that would yield grounds for rejecting obligations to the state. Oddly, however, his own theory of judiciously applied "trust" poses a substantial threat to independent judgment. He distinguishes between "rule-reliance" and "special characteristic" types of trust. "Both . . . involve anticipation or prediction. The first . . . is based upon the recipient's stated intention to do something in particular, the second upon the recipient's intention to act in a certain way." In political contexts the first translate into fidelity to law. Insofar as we are able to trust those in authority to act in particular ways, this is because there is a set of rules that requires that they do so. Now it is true that limitations on oversight and enforcement leave us little choice but to "trust" our rulers to respect the rules. But this is far too attenuated a sense of trust to serve Abbott's theoretical purpose. To trust someone is precisely to think that elaborate rules and mechanisms of enforcement are unnecessary. (Abbott instances students who feel an obligation not to cheat on an examination because the professor has shown trust in them by leaving the room. Would the students feel they had been trusted if the professor continually returned to the room or planted agents to report on cheaters? Would trust have been shown if the professor's absence was due to other business?)

The second sense of trust is at once more relevant and more ominous in respect to the autonomy of the citizen. In politics it arises from recognizing that those in authority have the wisdom, skill, or rectitude to decide issues that arise in a manner the citizen will approve. The advantage of this understanding of the relationship between the citizen and those in authority is two-fold. First, the official, as recipient of the trust, acquires a duty to be faithful to it. Second, the citizen has undertaken no obligation (expressions of trust are not performatives) and is at liberty to withdraw the trust when it is no longer deserved. Thus "one ought to obey the state to the extent to which he trusts those in political authority."

Abbott's objective in this argument is to find a conceptualization that makes continuing critical assessment of the performance of the state internal to the process of deciding whether to obey it. But the concept of trust is a poor choice for this purpose. Trust renders critical scrutiny unnecessary if not inappropriate. Moreover, Abbott exaggerates both the extent to which being trusted is conducive to trustworthiness (especially in impersonal relationships) and the ease with which genuine trust will be withdrawn when not deserved. Numerous liberals have thought that trust is exactly what ought never be placed in anyone who "has the shotgun behind the door."

But there is a more fundamental problem here. Trust must be a derivative not a constitutive feature of the relationship between citizens and those in political authority. Critical assessment of the actions of the latter may with time and good performance warrant a kind of grudging trust in them. But officials must have authority before they can warrant trust in themselves qua officials. Thus either there is no authority (in which case Abbott's problem disappears) or "trust" cannot be the basic concept in terms of which to understand authority and how to relate to it.

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Condorcet: From Natural Philosophy to Social Mathematics. By Keith Michael Baker. (Chicago: University of Chicago Press, 1975. Pp. 538. $22.00.)

Condorcet was a mathematician, philosopher, political pamphleteer and occasional politician, advocate of social rationality, and eventual victim of the Terror for his Girondist sympathies. Until quite recently he has been best remembered for his posthumously published fragment *Esquisse d'un tableau historique des progrès de l'esprit humain.* This title has become synonymous with the idea of progress and the infinite perfectibility of man. Recent scholars (notably Duncan Black, *The Theory of Committees and Elections,* Cambridge, 1958; Gilles-Gaston Granger, *la Mathematique Social du Marquis de Condorcet,* Paris, 1958; and G. Th. Guilbaud, "Les théories de l'intérêt général et le problème logique de l'agrégation," partially translated as "Theories of the General Interest and the Logical Problem of Aggregation," in *Readings in Mathematical Social Science,* ed. Paul R. Lazarsfeld and Neil W. Henry (Chicago: Science Research Associates, 1966) have rescued another of Condorcet's works, his *Essai sur l'application de l'analyse à la probabilité des décisions rendres à la pluralité des voix* from the neglect to which its opaqueness and lack of mathematical elegance have long condemned it (Baker, p. 227); and in my view it will prove to be the *Essai* rather than the *Esquisse* on which Condorcet's claim to distinction will rest in the long run.

The *Esquisse* is credited with considerable influence on figures such as Saint-Simon and Comte (the latter of whom hailed Condorcet as his "spiritual father" [p. 475 n. 4]). The *Essai,* on the other hand, can be regarded as the first work of mathematical political science, with direct influence on scholars such as Poisson and Cournot. It introduces two ideas of central importance to contemporary democratic theory: the notion of the paradox of cyclical majorities (which has been called by Guilbaud "L'Effet Condorcet") and the notion of a pairwise majority winner (now often referred to as the "Condorcet Winner") as the sine qua non of democratic choice. A third element of the *Essai,* in which Condorcet connects together the logic of majority rule with his probabilistic notions of truth, is at present less well known, but is crucial to an understanding of Condorcet's thought. Condorcet's jury theorem (which can be shown to be a corollary of the "law of large numbers") asserts that, for a group whose individual probabilities of reaching a correct judgment on some issue are each greater than one-half, the probability that the group's majority verdict will be correct approaches one as the size of the group increases—what appears to be a mathematical demonstration of a case of "vox populi, vox de." More generally, under certain assumptions, the Condorcet jury theorem permits a calculation of the tradeoff between elitist claims to special judgmental competence and majority rule.

Keith M. Baker's *Condorcet: From Natural Philosophy to Social Mathematics* is not a biography in the usual sense, but rather an intellectual history. Baker, an historian, provides a rounded portrait of Condorcet which, for the first time, links together the Condorcet of the *Esquisse* with the Condorcet of the *Essai*—relating Condorcet's philosophy of history to his "vision of a democratic society rendered rational through the power of social mathematics" (p. 370). Such an analysis is important because

> the treatment of Condorcet in the standard histories of sociology—such as it has been—has almost invariably rested on a brief characterization of the doctrine of progress found in the *Esquisse* as a powerful (if flawed) anticipation of the historical sociology established by his successors, Saint-Simon and Comte. Neglecting as it does the conception of social mathematics that has in recent years been recognized as lying at the heart of his idea of social science, this traditional view is clearly inadequate . . . and misleading (p. 344).

Baker is at his best in showing how Condorcet's views in the *Esquisse* have been misrepresented, misunderstood, and simply mistakenly rejected by those who claimed to follow in his footsteps. Baker's *Condorcet* is a work of thorough and indeed loving historical scholarship (nearly 90 pages of notes, over 600 references) which demands a fundamental reassessment of more traditional views of Condorcet, although its interpretation of Condorcet's career and views is not at variance with the basic outline ably sketched by Granger in his recent article on Condorcet in the *Encyclopedia of the Social Sciences.* In *Condorcet,* Baker discusses in detail virtually all of Condorcet's writings and casts important light on such neglected works as the *Tableau général de la science qui a pour objet l'application du calcul aux sciences politiques et morales.*

Condorcet was "remarkable, even in an Encyclopedic age, for the wide range of his interests and activities" (dust jacket). Anyone interested in the history of ideas; in the sociology of science; in the interplay between science, technology, and politics; in the origins of "modern" notions of social scientific explanation; and, indeed, in the birth of the idea of social science itself, will find *Condorcet* a treasure trove. Students of comparative public administration will find both amusing and instructive Condorcet's run-in with the ancien régime's department of bridges and roads. More generally, those interested in the social scientist as social engineer and in the complex interplay between gnosis and praxis will be fascinated by Condorcet's remarkable career(s). Those interested in the philosophy of social science will marvel at the "modernity" of Condorcet's views on the probabilistic nature of social science knowledge and on the role of mathematics as a tool for social understanding and social change.

Robespierre remarked of Condorcet that he was "a great mathematician in the eyes of men of letters and a distinguished man of letters in the eyes of mathematicians" (cited p. 383). Baker notes (p. 383) that "this remark had just enough truth to be really vicious." Baker's careful scholarship (particularly when combined with that of other recent work on Condorcet as social mathematician) gives the lie to Robespierre's canard. Baker's work is a major contribution to that "reevaluation of the whole of Condorcet's theory of politics" called for by Granger (cited p. 444 n. 4), which we believe is needed to provide the balanced portrait of Condorcet which that neglected and misunderstood figure justly deserves.

BERNARD GROFMAN

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The Efficient Organization. By Selwyn W. Becker and Duncan Neuhauser. (New York: Elsevier Scientific Publishing Co., 1975. Pp. 237. $12.50.)

The authors of this book want to say something quite simple: that the efficiency of an organization depends in part upon administrators knowing how well their subordinates work, a systematic gathering of information on the organization, and well-defined jobs appropriate to the situation. While this all seems obvious, it is not an unimportant intellectual task to demonstrate these points with empirical data. Unfortunately, on the way to the bank, something happens to obscure the obvious, to distort the findings, and to diminish the worth of the book.

The first part of the book, about 100 pages, purports to describe an entrepreneurial theory of organizations and a model of organizational efficiency tested in the second part of the book. This first part is an exercise in obfuscation which depends heavily upon the misuse of the English language. Simple things like how well administrators know their subordinates are performing translate into "visibility of consequences" (C_v); a good reporting system translates to "specification of visibility procedures" (PC_v); well-defined jobs appropriate to the task becomes "specification of production procedures" (P_j). Phrases like this lead to paragraphs like this: "Initiating structure (IS) defines the extent to which the supervisor delineates the situation for his subordinates to facilitate coordination aimed at goal attainment" (p. 57). At one time or another we have all been tempted to write grand theory. In most cases, colleagues, friends, and editors point out our follies. In the case of this book apparently none of these typical sources of criticism was consulted. In any event, criticism did not have much effect on the authors.

I hoped the second part of the book which seeks to test the theory of organizational efficiency put forth would be more clear. After all, numbers and formulas are supposed to be economical expressions of theory. Unfortunately, a number of elementary errors of judgment destroy whatever power the theory section possessed. Specifically, three errors occur in the data analysis section. First, the use of statistical tests of significance is inappropriate, given the non-random nature of the data; second, the variation of the dependent variable (efficiency) is restricted, and therefore the authors' claims that the model is powerful are misleading. Third, the stepwise regression model is used uncritically and leads to more misleading claims.

The theory to be tested is quite simple: organizational efficiency is a linear function of the level of administrator knowledge of subordinate performance, the extent of internal organizational reporting, and degree of job specification. At the last minute, organizational size and complexity are added, with little previous theoretical justification. So we have five variables predicting efficiency.

The empirical data to test the theory comes essentially from 30 case studies of hospitals in the Chicago area and 15 insurance companies in Venezuela. The authors never say how these organizations were selected but only that they were "30 medium-sized, short-term, not-for-profit, community hospitals in the Greater Chicago Area" (p. 99). Unfortunately they believe this is a "sample" which is "fairly typical of not-for-profit community general hospitals." It is not a sample of anything, certainly not a random sample. In reality, the data consists of 30 case studies of hospitals, and 15 insurance companies. Moreover, the organizations involved were selected, according to the authors, because they were very much alike, and provided the same type of service.

The authors perform a stepwise multiple regression on the hospital and insurance data respectively. They claim that the findings are statistically significant, the variables of their model explain more variance than other previous research on efficiency, their model efficiency is proved by the data, and that "the model is a good one not because of the amount of variance explained but because the model predicts that visibility of consequences should emerge as the most powerful explanatory variable" (p. 157).

All of this is misleading. The findings are not statistically significant because the data is not a random sample of anything. Significance tests *require* random samples (or some approximation thereof) in order to permit an inference from the sample to the population. No one can know if this model explains more variance than other models used with other data in large part because the dependent variable, efficiency, does not have much variance. The hospitals were chosen because they were so much alike; if the same model were applied to a wide variety of organizations, the overall variance explained would most likely drop. In short, where the dependent variable is of small variance, the power of the linear model almost always goes up. This is a product of the data, not a reflection of the power of the theory.

Last, I am always suspicious when people use a stepwise linear regression model because they usually are unaware of the implications. In stepwise regression variables are added in order of their predictive power (a forward step regression). The assumption is that the most powerful predictor is causally prior to the weaker variables. The authors make no case for causal priority (which is never proven by data) but rather celebrate the fact that visibility of consequences explains a lot of variance. This is largely a result of the regression method they use and of little or no theoretical importance *per se*.

Except for a reasonable review of the literature dealing with organizations in Part I, this book has little to recommend it.

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Shakespeare's Rome: Republic and Empire. By Paul A. Cantor. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 228. $12.50.)

This book is a study of *Coriolanus* and *Antony and Cleopatra* which holds considerable interest for students of political philosophy. While all three of Shakespeare's Roman plays "are certainly the most political of his tragedies" (p. 8), Cantor focuses on these two rather than on *Julius Caesar* to give coherence to his argument and to bring out most sharply the contrast between the Republic and the Empire, which he regards as the key to Shakespeare's understanding of "the phenomenon of Rome and Romanness" (p. 11). What, above all, makes the plays "companion pieces" despite their striking differences is that both "revolve around the same issue, namely, what is meant by being bound by the city and what is meant by being free of it." From different, though complementary, angles, both approach "the one problem of the relationship of the city and man" (p. 208). By showing how spiritedness is generally connected with the Republic and eros with the Empire and also what naturally results when one of these contrasting sides of human nature dominates the other, the two plays, Cantor argues, demonstrate what binds man to the city while at the same time urging him to live without it.

Cantor's interpretation of *Coriolanus* explains how the Republic's mixed regime generally harmonized the competing claims of eros and spiritedness so as to produce an extraordinary number of public-spirited men and yet conspicuously failed in the case of Coriolanus. With a sideward glance at the opposite character of the class structure in Plato's *Republic*, Cantor elucidates the connections Shakespeare draws between the mixed parties or classes within Rome's mixed regime and the fact that the city lacked a ruler in the fullest sense, and between this and the further fact that the warrior who most embodied Rome's ideals finally turned his spiritedness against the city and regime that had fostered it. Cantor thus examines Shakespeare's presentation of Coriolanus' and Rome's mutual banishments as tests of their respective claims to self-sufficiency and concludes that the playwright, judging these claims by a standard that "is not political in any ordinary sense of the term," finds both unfounded: the city and the hero alike remain bounded by or dependent upon convention or mere opinion; both lack "self-knowledge" or "wisdom" (p. 120, see also p. 207).

One wishes that Cantor had defended his contention (pp. 12–13) that Shakespeare dates the Republic from the tribunate's establishment rather than from the Tarquins' expulsion by at least some consideration of *Lucrece*. As it is, however, Shakespeare's work dealing with that expulsion is never even mentioned. This unfortunate omission does not detract from Cantor's excellent analysis of the Republican regime at work, but does raise questions about what principally constitutes Shakespeare's Rome and *Coriolanus'* place within it.

Antony and Cleopatra is a far more difficult play, and although there is much to recommend Cantor's interpretation, it nevertheless falls short of his interpretation of *Coriolanus*. Regarding the problem of fidelity as the central problem of both imperial politics and the love story in the play, Cantor ably shows how Rome's new political situation freed eros by removing the Republic's premium on spiritedness and changed the primary relationship of city and citizen into one of master and servent. In discussing the effects of eros' new freedom, however, Cantor stresses that Antony and Cleopatra's love "is a thoroughly unconventional relationships" (p. 158). No doubt it is unconventional in a politically important respect, but Cantor never qualifies its unconventionality. Unlike his indication of Shakespeare's transpolitical standard for judging the claims to self-sufficiency in *Coriolanus*, Cantor's interpretation of *Antony and Cleopatra* gives no sign of considering the unconventional in terms of "self-knowledge" or "wisdom" as distinguished from opinion. Instead, the unconventional is now seen as whatever occurs outside the law or is cosmopolitan in the conventional sense (cf., pp. 158 and 164). One result of this important, if tacit, change is that it becomes extremely difficult to see how Cantor can radically distinguish in *Antony and Cleopatra* between "being bound by the city and . . . being free of it." Indeed, what he calls the "transpolitical" differs only in size from the political and even the subpolitical (see, e.g., p. 188). A more general result is that the book as a whole loses its promised coherence as "what is meant by being free of [the city]" proves to differ fundamentally in its companion interpretations.

Offering a thoughtful presentation of Shakespeare's knowledge and understanding of the workings of Rome's contrasting regimes, *Shakespeare's Rome* is best when it is strictly political. The book has the substantial merit of showing that Shakespeare deserves to be taken seriously as a political philosopher.

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Shen Pu-hai: A Chinese Political Philosopher of the Fourth Century B.C. By Herrlee G. Creel. (Chicago: The University of Chicago Press, 1974. Pp. ix + 446. $14.50.)

Since 1949 American political scientists have devoted a great deal of time and research toward understanding the governmental institutions of the People's Republic of China. But they are less interested in developments before 1949 and tend to ignore completely Chinese institutional history during the almost 2,000 years of the imperial period. This is unfortunate for several reasons. Such important political institutions as civil service examinations and systematic rating of officials probably had their beginnings in China. And there is no doubt that to understand properly the insistence on confession of faults by present government officials one has to know something of the history of this idea in Chinese law.

For the past 20 years Herrlee G. Creel of the University of Chicago has been researching the origins of Chinese governmental institutions. His several publications on this subject can be found in the bibliography of this book. Now, in Shen Pu-hai, a Chinese philosopher who lived in the fourth century B.C., Creel believes that he has found the person who first clearly enunciated many of the principles on which the later Chinese administrative system was to be based. Why then, if Shen's ideas were so important, have they not been presented before? One reason is that his book, the *Shen-tzu,* is no longer extant. Creel has succeeded in bringing together at least part of Shen's writings from quotations in other writings. The second section of this book consists of these fragments given in Chinese with an annotated English translation, together with a concordance to the Chinese text.

The general reader, however, is most likely to be interested in the first part of the book where Creel expounds Shen's ideas and traces their influence on later philosophers and on Chinese institutional development generally. Shen was primarily concerned with the problems of developing a bureaucracy, with the organization and techniques of governing. He seems to have been indifferent to such problems as insuring that government was in the hands of the virtuous, which was the primary concern of Confucius and Mencius. Shen was occupied with the problem of how to choose an *effective* person for a particular post, not with whether that person was morally outstanding.

Creel's arguments for Shen's ideas are forceful and clearly presented. The fragments that have been preserved certainly make one wish that his book had not been lost. I am not fully convinced, however, by all of Creel's claims for Shen. For example, Creel argues that the reference to the rectification of names in the *Analects* is mistaken and that Shen should be credited with this idea. Creel's argument is that this theory does not correspond with the rest of the *Analects.* But the *Analects* deals with such a variety of subjects that it is difficult to prove that any certain part should not be rightfully attributed to Confucius. Neither do I accept Creel's argument that Shen should not be classified as a Legalist. The term "Legalist" is after all a translation and the original Chinese term *fa-chia* certainly is broad enough to include such persons as Shen Pu-hai as well as Shang Yang and Han-fei tzu.

Despite these differences, I and others interested in Chinese political thought must be grateful to Creel for bringing together the extant fragments of Shen's work and thus making us aware that even so early as the fourth century B.C. the Chinese were already thinking about problems relevant to the great bureaucratic system which was to govern China during the 2,000 years of the imperial period.

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Political Myth and Epic. By Gilbert Morris Cuthbertson. (East Lansing: Michigan State University Press, 1975. Pp. xxii + 234. $10.00.)

Cuthbertson performs a valuable service for "quantitative . . . 'social scientists,' . . . positivists, behaviorists, and materialists" (p. 221) by calling their attention to the value-laden political role of myths. "Myths," he argues, "determine behavior, economics, and social structure" (p. 221) by creating the "moral consensus and prohibitions" (p. 2) which structure a political community. Cuthbertson thus implies that those who have ignored the political role of myths and epics have contributed to the reduction of political science to a sterilized "science of behavior" which is simply divorced from those moral concerns which are the very essence of politics.

It is evidently this implied challenge to the still-dominant current of American political science which leads W. Y. Elliott to describe Cuthbertson's book in his foreword as "one of the most pathbreaking and original books on the history of political theory that has been published in many years" (p. xi). However, for those who never forgot that politics is informed by morality, Cuthbertson's book neither breaks new ground nor displays any notable originality. Indeed, the book fails even to pose those *questions* upon which an original contribution to the study of myths could be made.

Cuthbertson notes in his Introduction that the word "myth" may be understood so expansively as to become virtually synonymous with "literature," or it may be understood much more narrowly to denote "a story about the gods in their dealings with man and nature" (p. 3). While the reader might then justly expect Cuthbertson to offer some explicit definition of what he understands by "myth," no such definition is offered; rather, the very plurality of definitions of "myth" apparently is taken to prove that any symbols which order society may be described as "myths." Hence, Cuthbertson evidently regards "Marxism, capitalism, nationalism, and constitutionalism" as concealed or reconstituted "myths" (pp. 11, 44). Is there then no difference between "myth" and "ideology"? Even if one regards "myth" and "ideology" as two forms of the articulation of political and ontological order, one would not be justified in forcing the two under one rubric. At least, it is not immediately apparent what essential similarity exists between stories which portray "the ground of being" as intracosmic gods and, for instance, those thought systems which portray human beings as subservient to the progressive and impersonal unfolding of history.

Just as Cuthbertson does not ask the questions which would lead to a specific definition of "myth," he does not ask the relevant questions about the relationship between myth and reality. When he defends "the truth of myths," he does so on the basis of the observation that "myths are meant to be believed and usually purport to be true" (p. 8). For Cuthbertson, myths are "true" for those who believe them. Yet, such a tautological conclusion ignores the implications of Cuthbertson's own observations that "the morphae of myths [are] more consistent than the pattern of history" and that "certain mythic themes are habitually retained" (p. 103). May not the very constancy of mythic symbols indicate that they speak, however compactly, of a relationship between human beings and nature which is not subject to historical change? May not perennial mythic themes point to the possibility that nature, and not history, is "the measure of all things"?

Cuthbertson's failure to raise these latter questions is related to an inconsistency in his view of the status of myth in time. On the one hand, he repeats the claim that myths structure or "condition" politics, "political behavior," and "social norms" (pp. 2, 157). Yet, he also claims that myths are conditioned *by* society and politics (pp. 12, 158, 160, 161, 165). While the former claim implies at least the possibility that the myth-maker may articulate a truth independent of existing social and historical conditions, the latter view implies that the myth-maker is simply responding to social and historical forces. Ignoring the conflicting implications of these claims, Cuthbertson concludes his book with the claim that "freedom is myth's highest objective" (p. 209): his conclusion indicates that his failure to confront his contradiction stems from an ill-defined and undefended assumption that myths are located within a "pattern of history" in which there is a progressive development towards freedom and "rational liberty" (p. 211). It is no wonder, then, that Cuthbertson unquestioningly regards those myths which speak of the limits upon human life as "true" only in their own time.

This book's main value is that it catalogs an amazing array of myths, mythic themes, and mythological theories. Cuthbertson must be admired for his research, but repentant positivists and normative political theorists will find more value in the studies of myth by Eliade, Frankfort, and Voegelin.

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Modern Political Analysis. By Robert A. Dahl. Third Edition. (Englewood Cliffs, N.J.: Prentice-Hall, 1976. Pp. xii + 166. $8.95, cloth. $3.50, paper.)

To famous authors as to just rulers, tribute and honesty are due. Dahl's third edition (including a new chapter on political analysis) is a model of its sort—a readable, engaging, well-footnoted summary of abstract behavioralism. So much for tribute.

But what is the true purpose of a brief introductory text in our field? If it is the schematization of conventional positivist opinion, then this is a fine specimen. If not—if the object is to replace "information," premature model-building, and naive descriptivism parading as professionalism, with critical ingenuity, fact-scepticism, evaluation, and an ability to integrate—then this is not the book, and probably there exists no such book.

Admittedly, our texts are overly conceptual. Too often students are given the impression that our heuristic categories are naturally discoverable. We suppress the *connectedness* of social phenomena with bland warnings that the categories-to-be-memorized "shade off into" one another in some unspecified, metaphysical way. Thus a thoughtful student should not blame himself or herself for failing to grasp Dahl's (non)distinction between normative analysis (Ch. 9) and policy analysis (Ch. 10).

Worse, Dahl's book, like its competition, is not really about politics, except in the strained sense that a pharmacopoeia is "about" the practice of medicine. It is psychology without the psyche, referring frequently to an abstract "Individual" and to sanitized "Individual Traits," without conceding much of anything to feelings and values. It is given to using economic analogies before denying an essential connection between politics and economics—Chapters 1 and 2, for instance, cling to the quaint old view that socialism is merely an economic form.

But above all, the book is patently not about politics so long as politics has something to do with historical choice, creative activity, desire, courage, or substantive values. It cannot be about real public life or Dahl would have made more mention of human suffering and conflict, of which politics is widely thought to be either a subcase, a cause, a remedy, or all of the above. (Conflict, he analyzes, for example, in terms of "cleavages" and inconsistency of goals, seeing the government as a regulator, rather than a source, thereof.)

To what, then, are students being introduced? The long (Chapters 3 and 4) inconclusive discussion of 'influence' and 'power' suggests that they are being introduced to Academia, to mandarin skills that have precious little to do with political action. Otherwise, power and influence would have been presented as substantive problems and even (dare one write this?) as factors in an interpretation of the struggle for power in our own lives.

Nor is there any account of structural causes, nor any taste for concrete *explanation* as the aim of political analysis. Instead, Dahl's chief method is to hover over semantic issues. Occam's Razor requires, however, that fine distinctions be ignored unless they make a difference to theory and, derivatively, to empirical work and action.

Finally, one must confess that, like much behavioral political science, this book sometimes (e.g., pp. 66, 103, 105, 106) fails to soar to the level of common sense—as where (p. 66) it is revealed as an italicized insight that *"all political systems undergo change."* Graver is the lapse in the chapter (10) on policy, where the central issue of what *is* and what *ought to be* "maximized" is deftly evaded.

Yet these are problems for all our texts, haunting the profession as now constituted. It would be unjust to let such objections detract from the strength of Dahl's third edition. It would be far worse to mistake his fastidious intellectualism for ethical seriousness.

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Rousseau's Political Philosophy: An Interpretation from Within. By Stephen Ellenburg. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 335. $15.00.)

Studies of Rousseau, even when they are based on careful and elaborate scholarship, are never merely scholarly. *Rousseau's Political Philosophy* is no exception. It makes a contribution to political theory on two levels. First, it is a book which will partly assist in the current discussions among political scientists working to develop a participatory democratic theory by suggesting ways new theories of participation can draw upon Rousseau's work. Second, it is a book which presents a vigorous interpretation of Rousseau's theory of political culture and argues that Rousseau's principles of political legitimacy cannot be properly understood apart from his conceptions of virtue, sincerity, and *moeurs* (morals-manners). Too few books on Rousseau's political thought attempt, as does Ellenburg's, to work intrinsically through Rousseau's concepts of culture, moral psychology, imagination, and action to a criticism of his theory from the perspective of what, in his own terms, would be a political consciousness. This is an important task. The challenge facing the next generation of Rousseau scholarship is, I think, to construct a Rousseauian critique of Rousseau. Ellenburg has taken us a step in the direction of such a critique. However, I think that he oversimplifies Rousseau's understanding of the preconditions of political legitimacy and the relationship between political authority and social reciprocity or interdependence. Ellenburg understands very well the conceptual tools with which Rousseau attempted to recover the possibilities of political community in the modern world, but he misstates, or understates, the demands and outcome of that attempt.

The predominant concern of Ellenburg's argument is to clarify Rousseau's understanding of the cultural and institutional preconditions for political community. The first section of the book makes an insightful and persuasive analysis which distinguishes Rousseau from both utilitarian and contractarian liberalism. It shows how Rousseau's notion of political culture is distorted by an analytic distinction between individualism and collectivism or by the liberal distinction between public and private, state and civil society. This analysis is suggestive of the theoretical continuities between Rousseau and the early Marx. Subsequent chapters are devoted to an extensive, but sometimes annoyingly glib, exegesis of Rousseau's political writings.

Ellenburg's central thesis is that Rousseau's political thought is unified by two central clusters of ideas which Ellenburg calls "nonindividualism" and "anarchism." Nonindividualism involves Rousseau's rejection of the claim that an abstract, ahistorical conception of human interests or dispositions can serve as a starting point for political theory, and his assertion that the "artificial" or cultural self can only be understood within the context of specific institutionalized forms of interaction. Anarchism is Ellenburg's term for Rousseau's conception of political sovereignty and legitimacy. It involves the claim that political community must be institutionalized as a direct, participatory democracy in which there is no distinction between those who rule and those who are ruled. He argues that this "anarchistic impulse" explains Rousseau's demand for absolute liberty (Ellenburg calls this "negative liberty," a term which, in its application to Rousseau, will probably astonish Isaiah Berlin) and his rejection of theories of sovereignty based on representation. Here I think Ellenburg's emphasis simultaneously underestimates the demands Rousseau places on human action and consciousness in order that sovereign authority be generally shared by, or participated in by the members of a political community, and overstates Rousseau's unwillingness to tolerate and to justify the exercise of political power by citizens and by elected officials.

Consider the following formulation of this anarchistic principle:

> The persistent requirement within Rousseau's political thought is absolute "negative" liberty for all men [read: males]: each man must be completely independent of the will of every other. The only legitimate direction is unmediated self-direction, for one man is never entitled to command another, nor is one ever obligated to obey another.... Now when the writings of a political theorist argue that every exercise of political authority by some men over other men is incompatible with liberty and constitutes slavery, and that there is no alternative to liberty except slavery, and that those who rule are themselves enslaved because they rule others, then the reader is in the intellectual company of an anarchist.... Many passages in Rousseau's writings, especially if examined in isolation, appear to justify political authority for some men.... Sometimes Rousseau simply could not sustain the imperative of his anarchism. He bluffed. He played verbal tricks. Ultimately we shall have to examine his bluffs and tricks, for they confirm his insistence upon absolute liberty (pp. 118–21).

We are not told specifically which passages are to be second guessed as bluffs and verbal tricks, and it is not clear, at least to me, where in the book Ellenburg returns to analyze them. In any case, one has no difficulty citing not only passages but arguments in which Rousseau fails to "sustain the imperative of his anarchism." The reason for this is that the formulations here labelled "anarchism" contain conceptual confusions and simplifications. Rousseau has his confusions and simplifications, but these are not among them.

The first question one wants to ask is: in what sense does Rousseau argue that "each man must be completely independent of the will of every other"? Following Ellenburg's own explication of Rousseau's nonindividualism it is clear that on one level the reciprocal interdependence of human wills is constitutive of all forms of social and cultural life. Therefore, the properly Rousseauian question becomes, not independence or dependence, but what form of interdependence, what structure of reciprocity, defines the political community and distinguishes it from alienated forms of modern civil society? This distinction between political community and *société*, or, as Rousseau sometimes puts it using Aristotle's language, between association and aggregation, depends upon a prior distinction between two modes of consciousness and intentionality, the general and the particular will. Ellenburg discusses Rousseau's distinction between the general and the particular as modes of social interaction but neglects to analyze the way in which this distinction operates within the structure of each individual's consciousness and volition.

Can we conclude then, as Ellenburg goes on to do, that legitimate sovereignty excludes the activity of ruling and the concomitant exercise of power in a political community? Here again Rousseau's position is more complex than Ellenburg's interpretation suggests. In one important sense rule is excluded: no member of the political community acting on the basis of his general or political intentionality can be said to "rule" any other member who is acting on the same basis. However, significant scope for patterns of rule remain. First, Rousseau projects the traditional language of ruling, power, and authority onto the internal psychic condition of each individual: the general will represses the particular will. Second, the general wills of others can legitimately override the particular will of anyone. Third, officials have the special function of exercising power in order to implement the dominance of the general will over the particular wills of the citizens when the assembly is not active.

Rousseau is important in the history of democratic theory not because he eliminates the distinction between ruler and ruled, the concept of authority, and the problem of social control, but because he recognizes that in a truly democratic community these concerns are displaced into the mind and imagination of each citizen. It will mislead contemporary theorizing of democracy to make the mistake of failing to see beneath Rousseau's theory of participation a theory of power and to read into him, as I believe Ellenburg tends to do, a prescription for communal spontaneity where social life never engenders conflicts between rational selfishness and political responsibility or where shoes never pinch.

BRUCE JENNINGS

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The Politics of Rational Man. By Robert E. Goodin. (London and New York: John Wiley and Sons, 1976. Pp. v + 210. $14.95.)

This very ambitious book attempts to combine a particular conception of the nature of man (viz. a rational, utility maximizer) with a set of "norms of political morality" with the intent of addressing fundamental normative questions. Moreover, the author takes this mixture of what is often termed "positive" (non-normative) theory and explicitly normative theory and applies it to empirical questions, especially "the politics of environmental protection." Such a wide-ranging book almost necessarily contains flaws. Nonetheless few of us have the courage to tackle such an important and wide-ranging an effort as Goodin.

The author approaches his tasks sequentially, first laying out the rational choice framework. This section rests, of course, on the rationality postulates. One restrictive and self-admittedly crucial assumption is Wicksteed's principle of "non-tuism" which, Goodin states "requires that individuals do not take into account the interests of those *with whom they deal* in constructing their own preferences" (p. 13). Thus, the welfare of others has no effect on the citizen's preferences. The major additional structure is that of game theory. One might raise some objections to the analysis in this section, but few would quibble with the plausibility of the only really important conclusion drawn from this section "that, under certain specified conditions, rational men would generally find cooperative coordination preferable to either independent action or domination" (p. 63).

At this point, Goodin introduces his three "Fundamental Principles of Political Morality," in part to pick specific solutions to the general (and generally indeterminate) cooperative coordination game "by focusing on *fundamental principles of political morality by which rational man would choose to be guided*" (p. 63, emphasis added). The first is termed "the Aggregative Principle [and is defined as urging] maximization of the total stock of goods available to society as a whole" (p. 63, where "goods" is not restricted to material goods). Thus, this principle is essentially equivalent to, or at least sets the stage for, the usual Pareto Optimality condition. So saying, we come to the crux of the issue, how are these goods to be distributed? Usual (especially "positive") theories of rational men founder at precisely this point. Two principles are posited. Goodin states, "the fundamental distributive rule of a society of rational men is the Works Principle. According to this principle, each individual should prosper according to his own works" (p. 64). Finally, there is the Fairness Principle. Goodin posits a "hypothetical baseline in which no one's activities affect anyone else" (p. 64). Then, any change from that baseline is made if and only if appropriate compensation is paid.

I find these principles not completely unproblematic. First, I remain unconvinced that these principles are the ones rational men would necessarily choose. Goodin claims only that he can connect rational choice to the Works Principle with assumptions that "seem to be broadly true for individuals acting in isolation" (p. 79). The other principles can only be justified as those a rational man might choose in a highly anonymous society with nearly complete uncertainty, especially with respect to power relations. Whether these principles are applicable as practical guides to action in real societies where man is neither in isolation nor completely uncertain about power relations remains to be demonstrated. Of more importance, there may be some questions raised about the distributive principles. Who is to judge the works principle? If man is to prosper according to his own works, then comparative judgments as to the relative value of a man's works viz-à-viz others' works must be made. The working of the Works Principle is not self-evident. As to the Fairness Principle, the hypothetical baseline where "no one's activities affect anyone else" seems quite unclear to me. Again, as a practical guide to action which these principles are clearly intended to be, I find it difficult to imagine how that baseline might be established. Essentially, society seems to me to be based on interdependent activity; indeed, many activities are performed precisely because they affect others. Without a baseline, it is impossible to establish appropriate compensation even though "each man is taken to be the sole authority on his own welfare" (p. 64) and hence his compensation.

The hypothetical baseline problem affects part of Goodin's empirical analysis. The section on environmental policy is too long and complicated to comment on here and is commended to the reader. Goodin also applies his analysis to civil and criminal law, noting that criminal law only partially reflects fairness because it involves punishment as well as compensation (even though punishment may have a rational choice basis). He is on stronger grounds when dealing with the law of torts. However, torts, especially libel laws, involve punitive as well as compensatory awards. More importantly, torts attempt to reestablish the *status quo ante* through compensation which is not the hypothetical baseline of the Fairness Principle. Indeed, laws are often argued to affirm the existing status quo, especially in respect to existing power relations in society.

While much of this review has focused on what I consider to be the weak points in the analysis, there is much to recommend this book. It tackles a wide-ranging set of interesting and important problems. Goodin's analysis falls into the "Rawlsian tradition" and as such shares many of the same problems, but at the same time shares its promise.

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Verbal Behavior and Politics. By Doris A. Graber. (Urbana: University of Illinois Press, 1976. Pp. viii + 377. $12.95.)

This valuable book summarizes systematically the methodological worries, conceptual distinctions, and main empirical findings which have emerged in the study of oral and written communications in political behavior. It is divided into two sections: Part One is conceptual and methodological, devoted to reviewing the importance of verbal behavior, the constructing and encoding of messages, functions of verbal behavior, the mine field of making valid inferences from verbal behavior, and the problems and techniques of analysis and measurement. Because it is so frequently technical, this first section will probably have fewer interested readers than the second section.

The second section expertly reports and summarizes the important ideas and empirical results in five areas of concern to political scientists. Much of this material is brought together here for the first time. The five areas are: the definition of reality through the mass media (with particular focus on bias), verbal behavior of political elites, verbal behavior in public assemblies, in small bargaining groups (including international negotiation), and the concept of affect-laden condensation symbols. These chapters are the finest part of the book; they provide an efficient overview for both undergraduates and more advanced researchers.

The author, who has previously contributed empirical studies in both international and domestic communications research, provides numerous useful examples to illustrate her theoretical points. The broad scope and exhaustive command of relevant scholarship are unique and impressive. Extensive footnotes and a 29-page bibliographic essay will be a valuable resource for researchers. Taken together with a similar bibliographic effort in Dan D. Nimmo's recent *Popular Images of Politics,* which complements Graber's cursory attention here to dramatistic, mythological, and symbolic interactionist theory, the interested researcher will now find no lack of organized guidance to further reading in this complex, disparate, and developing field.

Verbal Behavior and Politics is essentially organized as a functionalist account and description of potential effects. Graber's model posits eight potential functions of verbal behavior (p. 44): attracting attention to political situations, creating political linkages and definitions, creating and maintaining conceptual straightjackets ("reality sleeves," as she terms them), making political commitments, producing policy relevant moods, stimulating nonverbal action, using words as action, and using words as symbolic rewards. Beyond this organizing framework for analysis and exposition (and the exhaustive treatment of the theories of others), there is not much original model-building of cause-effect theory.

Part of the problem can be attributed to the great diversity of the subject and the materials Graber has to work from. There is no Kuhnian paradigm in this field (except perhaps for the usual shared undercurrent of desire to expose, label, and correct deviations from a vaguely liberal, cooperative, enlightened-rational-discourse, democratic, internationalistic, normative image of how the world ought to work). This is an area of inquiry where rigorous model-building, testing of alternative theories, theory rejection, an organized common concern year by year to increase the proportion of explained variance of an important dependent variable, have been relatively absent. The path-breaking RADIR studies and Deutsch's work focused major efforts on stability, integration, and revolution; Edelman was partially concerned with political quiescence. But many other studies have been piecemeal, lacking the instinct for the jugular of an important distributive outcome. Studying processes instead of outcomes has often led to scholasticism, or at least to a bureaucratization of inquiry where a vision of the whole was lost, and isolated subproblems with vague relations to building the discipline increasingly detracted attention from R^2. The principal character of this book is that it is an exhaustive, systematic summary—ably done—but it is not broadly critical of these failings. It lacks a well-articulated map of the requirements for the field and its relation to a vision of the discipline. The author's intent seems to be to promote rather than to direct the field.

Previous studies of verbal behavior have tended toward theoretical regression in two ways. The first, forgetting about final outcome variables, I have already mentioned. The second dreamlike, aimlessly topical, isolated research arises because rigorous theorizing is more characteristic of this topic in psychology and (to some extent) sociology, and clarifying theoretical distinctions have tended to become lost or ignored by political scientists. For example, by concentrating on political scientists' findings, Graber's book becomes psychologically thin in places (e.g., on the theory of attitudes or Kohlberg's work on syndromes of moral verbalization) and leaves to the bibliographic essay Basil Bernstein's important sociological work on different language codes in different sections of the population. The isolating definition of political science as a social science discipline different from sociology and psychology (and even foreign to the theories of creativity and esthetic response in the humanities) has ill-served researchers in this area of inquiry. Empirical research with vague or degenerate psychological theory has not often been a very inspiring cumulative contribution to the social sciences. The result, as in this book, is a review of theories which are muffled and do not clash, research traditions passing by one another, a lack of tension and common adventure, topicality rather than the sense of problemicity which is a prerequisite for cumulative theoretical advance. At least Graber's book will provide greater common knowledge of what other people have done.

Summary charts, diagrams, maps like M. Brewster Smith's (reprinted in Greenstein and Lerner, eds., *A Sourcebook for the Study of Personality and Politics*) would have helped this book, since it is dealing with complex material. The summaries at the end of each chapter are well-done, but an inexperienced reader can get lost in the verbal presentation of different researchers' concepts of three types of rhetoric, four types of language, eight functions of language, ten possible "operational code" inferences from language, five role types, four motivational types, three subjective dimensions, two models from psychiatric game analyses, four types of oratorical drama, twelve interaction process functions, etc.

No reviewer could fail to acknowledge warmly the many years of thorough, competent, and exhaustive scholarship that are embedded in this book. It is unique, will be the standard text in the field, and many future scholars will be greatly in Graber's debt.

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Causal Analysis. By David R. Heise. (New York: John Wiley and Sons, 1975. Pp. vii + 301. $15.95.)

David R. Heise's *Causal Analysis* definitely fills a gap in the available literature on causal modeling. There is a rich collection of works for relatively unsophisticated readers and a large body of econometric and psychometric literature requiring substantial mathematical background. Because this work falls in between it may prove to be valuable. It is supposedly directed at practicing scholars, students (presumably upper-division and graduate), and interested lay persons. For the most part, it is intended as a supplementary text in methodology and data-oriented statistics courses. However, given the extent of involvement necessary in using the book, it would be more appropriate as a primary text, supplemented by relevant articles, in a course following a statistics course.

Organizationally, the book is straightforward with six detailed chapters: (1) Causality and Causal Analysis, (2) Causal Diagrams and Flowgraph Analysis, (3) Statistical Concepts, (4) Path Analysis, (5) Identification and Estimation, and (6) Dynamic Considerations. These chapters could easily constitute an entire quarter's or semester's work.

The conceptualization is less straightforward, although if given adequate attention, it is useful. The first chapter carefully and exhaustively defines causality, in the context of causal operators. As an example, "A material structure or structured process that implements a causal relation is called an operator" (p. 6). Ordinary modeling deals with variables and causal diagrams. Heise replaces "variable" with the concept of a "flow." The following is a sample:

> One way that the analysis of flows differs from ordinary causal analysis may be illuminated by considering an example in which A and B are two distinct causes of E. If A, B, and E were ordinary events, then the presence of A or B or both would produce the same E as either A or B alone. On the other hand, suppose that A, B, and E are flows. Now A or B alone implies E, but the presence of A and B together does *not* imply the same E. Rather, another flow, E', is implied. This new flow is an augmented version of E. With flows the operation of multiple causes produces cumulation of effects (p. 19).

Heise also works with flowgraphs which are a "special kind of mathematical formulation which represents equations in iconic form" (p. 38). Structural equation models are introduced and motivated through the use of flowgraph analysis rather than the more common direct presentation of equations.

This book has major strengths. One is the stress on the development of causal thinking and theorizing, and the relation of such theory to statistical interpretation of data. Another is a fine section on statistics and regression. Finally, the author devotes some attention to the problem of selecting coefficients for model interpretation. Overall, the book presents a complicated subject in a competent manner.

Nevertheless, potential users should be aware of several problems. The book will be used most effectively by an instructor with a very strong background in the philosophy of science, logic, statistics and methodology. Even advanced graduate students will not readily understand the material without substantial assistance since the text is not self-explanatory.

Heise purports to supplement statistics and methodology texts with *Causal Analysis*, yet his terminology is not integrated with that of any particular text and it deviates substantially from most presentations. At the same time, *Causal Analysis* cannot be used along since the examples and empirical applications are inadequate. At crucial points, the reader is directed to continue with econometrics texts which utilize different terminology. Thus the average reader who seeks assistance in solving a practical research problem will not find such assistance.

The presentation is heavy with jargon, as illustrated by "flows" and "operators," and uneven in its assumptions about the reader. The jargon and the cursory treatment of mathematical derivations assume some sophistication and would make the book difficult to use as an introductory text. However, as a more advanced text, it would fall short by giving data analysis short shrift. Some econometrics texts go further than Heise's without using matrix algebra.

Finally, the more sophisticated reader will recognize certain specific omissions and shortcomings. The discussion of identification does not refer to the most important work on the subject in econometrics by Franklin Fisher. It is not clear how the presentation of the conditions for identification could be extended through the use of alternative assumptions. There is no discussion of the problem of using standardized structural equation coefficients, or path coefficients, with the two-stage least squares estimation technique. Finally, the entire subject of the probability distributions of parameter estimates, i.e., confidence intervals for coefficient estimates, is not considered, especially the effect of multicollinearity on such confidence intervals.

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The Political Thought of Saint-Simon. Edited by Ghita Ionescu. (London: Oxford University Press, 1976. Pp. viii + 245. $16.25.)

Political modernization, social science, a unified Europe, functionalism, the need for a secular religion, organization theory, the coming diffusion of power—these are only a few of the themes that Saint-Simon dealt with before his death in 1825. Unfortunately, few of his writings have previously been available in English. In the present volume, Professor Ionescu attempts to remedy this; it cannot be said, however, that he has done this very successfully. To begin with the annoying things and then move on to more serious matters: the long Introduction abounds with errors—typographical, grammatical, and stylistic; moreover, a key passage incorporated in the Introduction (p. 56) is translated significantly differently from the same passage when translated as one of the extracts (p. 215). More importantly, Ionescu seems to think that Saint-Simon can be made useful to us only by purging the writings of their "normative, utopian ballast," by "weeding out the extravagant allegoric examples," and by eliminating the "circumstantial and propagandist portions" of the work (pp. 12, 59). The justification for this, Ionescu tells us, is that Saint-Simon's work thereby gains "clarity and coherence" (p. 12). But is it Saint-Simon? In a curious way, this is not a question that interests Ionescu. What does interest him is the industrial-technological society (he has written a previous book on the subject), and he suggests several times that his interpretation (and editing) of Saint-Simon may be "anachronistic." "Interpretations of works of social philosophy depend on the perception of the interpreter in his own environment, and are therefore open to suspicion. . ." (p. 5).

Both the Introduction and the texts selected for translation focus on Saint-Simon as the analyst of the industrial-technological society—its organization, functioning, and institutionalization. The emphasis further is on the processes of public decision making ("politics" as Ionescu understands it) in a world where the "politics of power" is outmoded by the "politics of abilities" presided over by scientists, artists, and industrialists—by those, in short, who possess functional competence. Henceforth, politics can be reduced to administration over things, power will neutralize itself by its own diffusion, and ideology ("Jacobinism") will disappear or be suppressed as a disturbing and unnecessary element in the new society. Nevertheless, in a society geared only to the production and consumption of material objects, Saint-Simon is aware that a moral and spiritual vacuum will occur. What is required, therefore, is a new religion—one that will have its own morality, its own form of worship, and its own dogma. Surprisingly, Ionescu views Saint-Simon's "New Christianity"—a political concoction tailored to the needs of industrialism and social unity—as an attempt to restore "Christian principles and values as the basis of social life" (p. 56). Ionescu does, of course, have reservations about Saint-Simon—he failed "to visualize the ugliness and dehumanization which has overtaken industrial life"; he was unable "to grasp the importance of capitalism" (p. 52). Nevertheless, Ionescu suggests that Saint-Simon's model of industrial society is far superior—firmer conceptually, more comprehensive—than anything to be found in Marx or Marxism. Hence, on this interpretation, Saint-Simon's relevance to contemporary political thought and society.

ROGER W. SMITH

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Toleration. By Preston King. (New York: St. Martin's, 1976. Pp. 239. $16.95.)

The aim of this book is "to explore thoroughly the logic, implications and applications of the concept of toleration" (p. 16). The author writes with elegance, he is well-informed, and he is ingenious in analysis. Most political theorists who read the book will find things of value, such as the author's definition of tolerance as consisting in a "double-negative" (p. 37)—an objection to a statement or act, which is negated with the statement or act thus being accepted. They are apt to find numerous pages of interesting analysis and thus likely to feel that the author has not altogether failed in his aim. Whether he has altogether succeeded, however, is another matter.

The best political theory is apt to be complex in argument yet simple in fundamental insight. It is this simplicity that is lacking from *Toleration.* The book is filled with careful and exhaustive arguments but it is difficult to determine what they all come to. One finds no governing vision of toleration and its place in human society. Discussion is burdened by analytic intricacies which momentarily fascinate but in the long run bewilder.

Some readers may gain the impression that the author's governing vision is that of a society in which the people are enlightened by a toleration so ample and so responsibly used that they are able to maintain a clear ascendancy over all groups seeking exceptional privileges and powers. There are some eloquent pages on this subject in the final chapter. Yet in general the author shows a definite tendency to devalue tolerance. It is, he asserts, "value-neutral" (p. 67); its worth depends on what is tolerated. Tolerance is only a means, not an end. If the end is truth, other means may be equally important; moreover, truth itself is not absolutely good. So far as the relation of tolerance to popular sovereignty is concerned, the author in some passages treats tolerance as a concession on the part of a superior group and thus as having no place in a true democracy. Hence when the democracy he advocates is once established, tolerance will be anachronistic.

True, King indicates that *toleration*, as distinguished from *tolerance*, has a necessary place in democracy. But he does not seem to hold consistently to this view (for example, p. 15, "we can only dispense with toleration . . . in direct relation to the degree of feasibility . . . of democracy.") Further, the distinction between tolerance and toleration is illustrative of the intricacies which burden King's argument. Tolerance exists, we are told, only where the objection to be negated is unequivocal and certain. Toleration, in contrast, arises where the objection is weakened by equivocation or uncertainty, *or where the objection altogether disappears.* In the latter case one may positively favor the ideas and acts he tolerates. But is this toleration? One cannot positively favor all ideas and acts, and if one achieves toleration merely by permitting those statements and acts which one does positively favor, then apparently we must say that toleration abounded during the Middle Ages and Reformation and that it is exemplified in our own world by priest-kings such as Stalin and Hitler. If this inference rests on a misunderstanding of Professor King's argument, it is a misunderstanding which his words seem to invite.

Let me give another example of these burdensome complexities. As noted, the author excludes uncertainty from tolerance yet includes it in his concept of toleration. Further, however, he admits it into his concept of tolerance by a side-door labelled "procedural skepticism" (p. 126). Even though we are in no doubt about our objection to a statement or an act, we cannot avoid "a mere recognition of human fallibility" (p. 126). This surely is questionable. If our recognition of human fallibility is genuine, and not merely verbal, it presupposes real doubt. The argument becomes even more perplexing when the author asserts that the doubt on which tolerance (not toleration) depends may attach either to the initial objection or to the repressive action it suggests. In short, one may be uncertain not only whether one's objection is well-founded but also whether that objection calls for an act of repression. The difficulty is, however, that one may be absolutely *certain* that one's objection does *not* call for an act of repression (as the author explicitly recognizes in discussing Christian tolerance) and then tolerance, confusingly, does not after all depend on the uncertainty which has been so confusingly introduced into the concept.

A final example: King asserts that tolerance of ideas is concerned primarily with reaching the truth and that tolerance of groups and organizations with reaching justice. This is a neat, and thus tempting, correlation. But it is not very helpful. Is it not clear that in deciding whether to tolerate certain ideas one may be concerned with justice, with community, and with various ends other than truth? Locke argued for tolerance of ideas on the grounds, among others, that it would further peace and order. Moreover, tolerance of ideas and of groups can seldom be separated. Most groups that prompt intolerance do so at least in part by virtue of their ideas, and most ideas that prompt intolerance are espoused by groups.

Numerous other examples could be given of definitions and distinctions which encumber the argument and obscure its conclusions. But King's most serious error seems that of being, without clear reasons, so reserved about the value of tolerance. Had he actually launched an attack on tolerance, that would have been interesting. Had he been skeptical of it for weighty reasons, that also would have been interesting. But that King should be so lukewarm about a subject which he has chosen for intensive thought, and that his reservations should be so seemingly casual and inconclusive, is merely puzzling. From the fact that tolerance is not an unconditional imperative, it does not follow that it is also not, on the whole, of great value. Helping others, for example, is not an unconditional imperative (we should not help robbers holding up a bank), but we do not on that account doubt that it is a worthy ideal. As for the value of truth, it is not placed seriously in question merely by assertions to the effect that it is no more than "one of several competing ends" (p. 137). King asserts that tolerance is not an end, for if it were we would have to tolerate everything. But to assert that something is an end is not to assert that it is the *only* end and is thus unregulated by its relation to other ends.

Mill's best pages express a vision of man seeking truth and common understanding and thus bound, despite compromises exacted by circumstances, to maintain a stance of tolerance. That stance becomes a particular mark of humanity. There are few signs of that vision, or of any other, in King's pages.

GLENN TINDER

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Humanomics: How We Can Make the Economy Serve Us—Not Destroy Us. By Eugen Loebl. (New York: Random House, 1976. Pp. 164. $6.95.)

Loebl's stated purpose in this short treatise is to begin the task of building a science of the economy which might help us direct our "spaceship earth" toward humane purposes (p. 3). While his purpose is surely commendable (if somewhat obscure), the results are more than a little disappointing. In fact, his arguments are so confused, muddled, and liberally adorned with unexamined assumptions, pious "thou shalts," contradictions, and Rube Goldbergish constructions, that one is tempted to ask why the book was published in the first place.

As best as I am able to determine, Loebl is disturbed both by contemporary economic science (mainstream and critical) and by the operations of actual capitalist and socialist economics. While there is much to be said for such dissatisfaction and much that needs to be explored along these lines, Loebl's alternative conceptions of theory and practice verge on the ludicrous. With respect to economic theory, he is disturbed primarily by the prevalence of "determinism," by which he means, strangely enough, the notion that there might exist some observable regularities in human behavior, or by the fact that we might use particular theoretical frameworks or concepts to aid our observation. To Loebl, such practices not only lead to the negation of the unique particularity of thinking human beings, but to dangerous oppressions as well. He argues, for instance, that the *concept* of ownership (whether private or social) has inevitably led to fascism, communism, World War II, the Cold War, and the balance of power (p. 11). The calibre of his theoretical argument rarely rises above this level. As to his critique of the operations of actual economic systems, there is not much to be said other than to point out that he merely rehashes conventional and already widely disseminated interpretations. He spends many pages, for instance, trying to convince the reader that a free market no longer exists in the mature capitalist economics, and that the laws of supply and demand do not work precisely as the neoclassical economists proposed.

What is at base so strange about this book is Loebl's inability to see the basic contradiction posed between his analysis of the nature of the problems of the modern economics and his desire to build a "humanomics." He proposes, on the one hand, that we reject the idea that economic life is comprehensible in a theoretical sense, that it is governed either by objective laws or observable tendencies, because such thinking is inherently oppressive. On the other hand, he proposes that since the economy is a human creation, we turn it over to human direction and guidance. However, in Loebl's conceptually and theoretically anarchist world, it is difficult to see what tools and guidelines are available by which human beings might give human direction to their economic life.

Devoid of theory, Loebl is reduced to preaching. Toward the end of the book, he presents a list of principles which, from his view, must undergird any humane system of economic life. While many of these principles are commendable, and certainly supportable, he never demonstrates their derivation; they appear literally full-blown from Loebl's imagination, with no theoretically convincing supporting arguments, whether scientific or ethical. His list of principles remains unconvincing, therefore, and no more likely to command support than anybody else's personal list.

From this list of principles, Loebl derives a bewildering series of "thou shalts" which he believes ought to be instituted by government (which he constantly and irritatingly calls the "macro-organ," suggesting some X-rated film), ranging from the redistribution of resources from the rich to the poor, to the imposition of human purposes on the economy, all the way to the favoring of small business over the giant corporations. All of this is to take place, by the way, free from interference into the affairs of private enterprises. He not only seems strangely unaware of the contradictions in these positions, but he proceeds to paint a picture of the political world in which such reforms are to take place that is completely unrecognizable: devoid of classes, of concentrated economic and political power, and of unequal resources. As Loebl puts it, it is not "vested interests" which stand in the way of a humane economy, but our inability to conceptualize the economy "as an integrated system of thinking human beings" (p. 143).

Given such incredible twaddle, I can only conclude that books such as this are published only because of a widespread notion that being a refugee from an Eastern European jail endows one with special wisdom. This book stands as a powerful reminder to avoid the temptations of such misdirected notions.

EDWARD GREENBERG

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Reinhold Niebuhr: A Political Account. By Paul Merkley. (Montreal: McGill-Queen's University Press, 1975. Pp. xii + 289. $13.50.)

This biographical volume is not an attempt at an all-encompassing treatise covering every facet of the hero's life, nor a painstaking study of his phenomenal influence over his generation, but a specific probe designed to complement prior publications concentrating on Reinhold Niebuhr's contributions to theology, political theory and international affairs. Its main purpose is to give *A Political Account* of Niebuhr's formative years with special emphasis on the domestic situation of the United States and his concrete political commitment. To provide a more thorough understanding of its strength and nature, Merkley goes beyond a purely descriptive treatment by taking a closer look at the primary principles which have inspired Niebuhr's political writing and actions.

The presentational matrix is provided by the main stages of Niebuhr's ideological development corresponding to the four parts of the book, beginning with the protestant-progressive ideas of "Bethel's Pastor" during the 1920s, to the "Apostle to the Left" of the 1930s, to be followed by the "Theologian of Crisis" of the lage 1930s and early 1940s, and finally, the period of the "Theologian of the Vital Center" during the remainder of his years.

In analyzing these phases, Merkley deals with many crucial domestic issues as they relate to the dialectic between Niebuhr's desire to improve conditions and the political realities of the American setting. He presents his thoroughly researched material in a delightfully multidisciplinary manner and in a quite readable and slightly argumentative style.

In between one finds a discussion of specific questions such as the attitudes of a second generation German-American, the influence of Protestantism in the political experience of the United States, the Marxist movement as seen through the eyes of Reinhold Niebuhr, the influence of German theologians, especially Karl Barth and Paul Tillich, Reinhold Niebuhr's meandering through the editorial field, and the uses and misuses of his reputation for political purposes.

Yet the overarching theme of the book is that peculiar though not altogether rare phenomenon of a serious and influential man being attracted to Marxist thought and then discarding the ideology just as Niebuhr had rejected capitalist-liberalism earlier in life. In a way it seems to be a sad story of youthful idealism being soured by age; the change seems to put either his earlier or his later writings and statements in doubt. And yet, Merkley argues that neither of these interpretations is valid because the apparent change is really a manifestation of a personal "integrity" due to a primacy of Christian principles (pp. viii and 234). It was the inherent limitations of both liberalism and Marxism with their excessive emphasis on selfishness of one kind or another, that failed to satisfy Niebuhr's needs for a strategy to accomplish political improvement. Not that he would deny the historical constancy of egotism; only the excessive and almost exclusive emphasis on it must be rejected. To do this, Niebuhr perceived historical events in a wider context of a transcendental purpose. He reaffirmed the existence of a paradoxical linkage between the seemingly meaningless misery and death of the individual, and the meaning of history. It was the fault of classical naturalism to distract men from their true realities by excessively stimulating their concern with the material well-being of the community as the ultimate frame of reference for individual meaning (p. 212). Human beings must be rescued from their consumer orientation to become active elements in the historic process. That humanity is naturally inclined that way is clearly indicated. "The fear of death is ... the clearest embryonic expression of man's capacity as a creator of history" (*Nature and Destiny of Man,* II, p. 8). But, in Niebuhr's opinion, it is only Christianity that recognizes the peril, as well as the creativity, of human freedom. "No philosophy of history will satisfy us unless it accounts for the performance of human selfishness as a fact of history, and at the same time delivers us from the despair which assails us when we reckon with the fact of selfishness as an obstacle to pure progress and bringer of possible chaos. An adequate philosophy of history must contain the promise of personal redemption from the consequences of selfishness" (p. 222). And this redemption must be respected by political action.

To have carried forth this linkage between the absolutes of Christian love and purposeful political action is, according to Merkley, the key to understanding Reinhold Niebuhr's ideas. Being a protestant preacher has tuned him in the more readily to what is really an American tradition, those *middle axioms* between Christian theology and practical statesmanship, some of which are democracy, the dignity of man, and equality before the law.

C. F. SCHUETZ

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Karl Marx on Freedom of the Press and Censorship. Translated, with an introduction by Saul K. Padover. (New York: McGraw-Hill, 1974. Pp. xl + 208. $3.95, paper.)

Karl Marx on Freedom of the Press and Censorship constitutes Volume IV of *The Karl Marx Library,* edited by Saul K. Padover. The three preceding volumes in the series—*On Revolution, On America and the Civil War,* and *On the First International*—were published earlier. As I have pointed out in my review of *On Revolution (APSR,* 68 [Dec. 1974], 1743), the usefulness of *The Karl Marx Library* is considerably reduced by the English language edition of the collected works of Marx and Engels, jointly prepared by Lawrence & Wishart (London), International Publishers (New York), and Progress Publishers (Moscow). The first four volumes of the English language edition, covering the years 1835—1845, are already available and Volume I includes most of Marx's journalistic work translated again in the book under review, and much more besides.

On Freedom of the Press and Censorship consists of four parts. The first provides a representative sample of Marx's articles written for the *Rheinische Zeitung.* The second is a disappointingly inadequate selection of Marx's contributions to the *Neue Rheinische Zeitung.* The third includes a shortened version of a few personal letters by Marx to his famous friends (with the signal omission of a letter to D. Oppenheim outlining the kind of policy *Rheinische Zeitung* could have most usefully carried out). Finally, the fourth contains reports, official letters and documents by and about Marx, all of them connected in some way or other with Marx's editorship of the *Rheinische Zeitung* and the *Neue Rheinische Zeitung.*

The purpose of the book under review of vitiated by two contentions and doubtful assumptions. The first of these is that one is able to distinguish between "Marx as Revolutionist" and "Marx as Embattled Journalist and Editor." On account of this distinction, every interesting piece of Marx's political journalism published during the Revolution of 1848 was allocated to the previously published volume *On Revolution.* According to the second assumption, Marx's main interest as a fighting journalist—whose days apparently ended with the suppression of the *Neue Rheinische Zeitung*—was the establishment of freedom of the press and the abolition of censorship.

It is true, of course, that Marx's earliest writings are mainly, though not exclusively, concerned with an impressive and incisive defense of the principle of the freedom of the press and the exposure of the absurdities and evils of censorship. But in the *Neue Rheinische Zeitung* this was no longer the case. As a matter of fact, except for his two speeches to the jury in his own defense, both of which were published in the *Neue Rheinische Zeitung* (but only one of them is included in the book under review), Marx wrote nothing of significance on those subjects at that time. As a political leader and journalist Marx tried to stem the rising tide of the counter-revolution, led by the King of Prussia and his ministers, who used censorship effectively, to restrict the activities of their political opponents and, finally, to silence the opposition altogether. Marx's fight against the use of censorship as a political instrument was part and parcel of his struggles with the Prussian authorities. The record of these struggles—police harrassment, arbitrary arrests, suspension of the *Neue Rheinische Zeitung* under martial law, indictments for incitement to rebellion, and ultimately and expulsion of Marx from Prussia—is well known. Historians' and eye-witnesses' accounts, including Engels', have related much more than a selection of extracts of purely factual content from the *Neue Rheinische Zeitung* could ever do. Thus, *On Freedom of the Press and Censorship* is neither a book of source material about the young Marx as a professional journalist defending freedom of expression and, having been exposed for the first time to social and economic problems in Germany, becoming a critic of German society, nor does it present a more mature Marx, the author of *The German Ideology, The Poverty of Philosophy, The Wage Labor and Capital,* and *The Communist Manifesto* who was testing and revising his social and political ideas on the revolutionary upheavals in Europe of 1848.

In his introduction, Padover provides an enlightening description of Germany at the time Marx became a contributor to and the editor of the *Rheinische Zeitung.* But I am afraid he is wrong in presenting the *Neue Rheinische Zeitung* as an organ of the revolutionary proletariat and its policy as one intended to transform a bourgeois revolution into a proletarian one. Apart from the fact that there was no proletariat to speak of in Germany at that time, it is enough to compare "The Demands of the Communist Party in Germany" (drawn up by Marx and Engels in March, 1848, as a selective adaptation of *The Communist Manifesto* to German conditions) with *The Communist Manifesto* itself. In his short article "Marx and the *Neue Rheinische Zeitung"* (1884), Engels wrote that the political program of the newspaper consisted of two main points: "a single, indivisible, democratic German republic, and war with Russia, which included the restoration of Poland." The actual program, as formulated in "The Demands of the Communist Party in Germany" was more comprehensive than Engels claimed but it tried mainly to rally the peasantry and the petty-bourgeoisie to the cause of a bourgeois revolution against the autocratic state, the big landowners, the aristocracy and the Army. Marx was criticized by Andreas Gottschalk, Wilhelm Weitling and others for not being radical enough and for telling the workers and their organizations that it was in their own interest to help the bourgeoisie achieve its political aims. Only when the development of events in Germany, Austria, and France showed that the bourgeoisie, and, especially, the German bourgeoisie, had neither the strength nor the determination to seize power in the state, did Marx change the editorial policy of the *Neue Rheinische Zeitung*. The change was dramatically formulated in a series of articles "Bourgeoisie and the Counter-Revolution" (December, 1848). After December, 1848, the German proletariat no longer appeared on the political stage—to use Engels' phrase—"as the extreme democratic party."

There is a serious misprint or mistranslation of the title of Marx's article "Randglossen zu den Anklagen des Ministerialreskripts" (1843) which appears under the English title as "Marginal Notes on the Changes [should be Charges or Accusations] Made in the Ministerial Rescript."

Z. A. JORDAN

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Fights, Games, and Debates. By Anatol Rapoport. (Ann Arbor: University of Michigan Press, 1960. 5th printing, 1974. Pp. 400. $4.95, paper.)

This is still a fine book, but it has been somewhat outdated. It is interesting and provocative reading for those who know little or no game theory and want to acquire a smattering, and for those who want to obtain some acquaintance with problems in modeling human behavior. However, there has been much written since 1960 in this field and in spite of the excellent writing, the incentive to read this book is no longer as great as it was.

Rapoport's later books are to some extent in competition with *Fights, Games, and Debates*. There is more game theory in the two books on two-person and n-person game theory; there is experimental depth in his book (jointly authored with Chammah) on the Prisoner's Dilemma and a new book on experimentation about to appear.

Rapoport has a considerable gift for apt analogy. His examples of conflict or cooperation are in general fascinating and enjoyable reading. Nevertheless, the author himself appears to have been too busy to update his book. There is not even a new foreword. If it is printed again, I urge the author to add a chapter or two. Such an addition could reset most of the text in the context of what has been learned about game theory in the last 15 years.

MARTIN SHUBIK

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Partisans of Freedom: A Study in American Anarchism. By William O. Reichert. (Bowling Green, Ohio: Bowling Green University Popular Press, 1976. Pp. x + 602. $25.00.)

American anarchism has been a weak and ineffectual political movement. Aside from a handful of colorful personalities and martyrs—Emma Goldman, Sacco and Vanzetti, and Haymarket victims—and an equally small number of original thinkers, American anarchism has been a largely invisible creed. This is somewhat surprising, given the hostility to government and the strong strain of individualism so often found in American life and thought. William Reichert's large book demonstrates that the landscape of American life is littered with a great many anarchists. Unfortunately, his prodigious and useful research into the innumerable anarchist journals and writings has resulted in a flawed book which confuses more than it enlightens.

Reichert writes of anarchism from a sympathetic and approving posture. Worried that its bad name arises from misunderstanding and an erroneous equation of anarchism with violence, he attempts to demonstrate its basis in a philosophy of freedom and free thought. By the end of the book, however, the reader may well wonder what anarchism, in fact, means. After asserting that its essence is free thought and hostility to religion, Reichert argues that the "very attitude of simple faith that characterizes the Catholic Workers Movement is anarchistic in essence" (p. 497). We also learn that someone was expressing anarchist leanings "when he argued that the American social structure is class-ridden" (p. 171).

The discussion of violence is indicative of the author's confusion and contradictions. After asserting that violence is "not a necessary principle" of any reputable anarchist (p. 2) and that rejection of force is a fundamental principle (p. 6), we are treated to a succession of notable anarchist thinkers who justified, encouraged or defended some forms of force and violence. Lysander Spooner justified the right to use arms against despots, Benjamin Tucker viewed violence from a utilitarian standpoint, and Johann Most published technical instructions on dynamite and bombs. Unifying these views was the belief that liberating violence was quite different than the violence visited upon the oppressed. Reichert, however, asserts in his conclusion that "all anarchists are totally pacifists" (p. 586). Are Spooner, Tucker and Most, then, not anarchists?

One of the author's goals is to put to rest the belief that anarchism was a European importation. In this effort. he succeeds, detailing the ideas of dozens of American anarchists. Unfortunately there is little coherence to the book, each chapter detailing the ideas and, occasionally, the life history of one person or group of people. In his search for anarchists, Reichert spends much time on people who merely repeated others' ideas and apparently had no influence on anyone else of note.

Partisans of Freedom is poorly written. There are dangling particples, misspellings and trite and hackneyed phrases strewn throughout the book. There are strange statements of fact which are simply untrue. Scholars who know something of the condition of Jews in Russia will be startled to learn that many Russian Jewish families living in the Pale of Settlement in the 1870s were well-off (p. 407). There are logical inconsistencies in argument (p. 40 on Come-Outer-ism).

Finally, there is an objectionable simplification in this book, illustrated by Reichert's dichotomization of American political life into libertarian and authoritarian camps, into the latter of which is lumped the social contract thinkers. By making such gross generalizations, Reichert minimizes the individualistic strain which is common to anarchism and liberalism and sets both off from traditional conservatives. *Partisans of Freedom* contains useful information about a score of poorly known anarchists but lacks a clear, consistent, significant theme and argument.

HARVEY KLEHR

Emory University

Adam Smith's Sociological Economics. By David A. Reisman. (London: Croom Helm and New York: Barnes & Noble, 1976. Pp. 270. $21.50.)

David Reisman adds another to the recent efforts to revise the standard pictures of Adam Smith as proponent of bourgeois greed or as precursor of technical utilitarian economics. (Cp. recent writings by Robert D. Cumming, Duncan Forbes, Hans Medick, Ronald Meek, and Donald Winch, as well as the earlier book by Joseph Cropsey.) He offers a coherent account, locating Smith's economic ideas within a comprehensive social theory. Although the design is interesting, centering on a notion of economic policy for growth as means to non-economic social objectives, the work is seriously flawed, and cannot in the end make good its claim of doing more than abstracting a model from Smith's writing. Smith might have reasoned as Reisman says he did; but the evidence is not strong enough to persuade me. We are provided a different picture, but not a better likeness.

At the outset, Reisman quite rightly challenges those who presume that the intellectual map of the eighteenth century provides for economics in the modern sense. Instead of studying the map himself, however, he is content with a conjectural geography, stated in language which testifies how much he remains a stranger to those places:

> Because of his anthropocentrism, he would have sought to integrate economic activity into the broad science of humanity which he (like Comte later) was trying to create.... The problem with Adam Smith is not simply where to locate a cosmopolitan intellectual of the eighteenth century in terms of the boundaries laid down by our modern maps, but also where he would have located himself. Here his view is clear: since academic overspecialisation can so easily lead to confusion, intellectual anomie, and the emergence of the *Fachidiot,* it is best not to locate oneself at all (p. 12).

Questions about the relationships among the two theories Smith wrote and the theory he never completed—the theory of moral sentiments, the theory of political economy, and the theory of natural jurisprudence—have to be addressed with a subtlety and care which Reisman does not marshal. Consequently he offers an enforced unification of Smith's complex thought rather than a compelling structural analysis.

Reisman contends that Smith has a "functionalist" theory of social integration, an "economic determinist" theory of social structure and social change, and an "historicist" conception of the historically limited relevance of any moral or political recommendations.

He persistently likens Smith's moral theory to Durkheim's, maintaining that the principle of sympathy provides motive power to a process of internalizing established social norms embodied in a collective consciousness. Reisman does not work through the structure of Smith's argument in the *Theory of Moral Sentiments,* perhaps because of his preconceived notion as to where such a study would fall within a theory like Durkheim's. Consequently, he disregards the whole section dealing with judgments of merit and demerit, while he exaggerates the importance of the fourth and fifth sections, which Smith introduced as qualifications to his main argument. The result is a profound distortion of the mix between universal humanist and historicist considerations in Smith's moral theory, as well as a failure to appreciate the important ironic overtones in Smith's rehearsal of teleological arguments—overtones which were not missed by Reid and Ferguson and others among his contemporaries who found him too epicurean.

The thesis that Smith builds upon an economic determinist theory of history was earlier elaborated by Ronald Meek and several of his students, as Reisman acknowledges. Reisman states it in very strong terms, contending that "social, political or moral factors . . . are mere dependent variables" (p. 137), and using the language of base and superstructure. There is an arguable case here, perhaps, but much of it depends on passages characterizing great periods of history rather than on Smith's distinctive detailed theoretical argument about morals or political oeconomy. There is no question that Smith saw mankind's involvement in the economic system as producers or consumers a crucial part of their experience and of the life of their society; but it is hard to speak of an economic-determinist historical theory when his theories, properly so called by his own criteria, are not for the most part historical, when the sequence of historical stages is barely comprehended in theoretical terms, and when the crucial theory of political and legal activity remains largely unwritten.

According to Reisman, Smith aims at a progressive state of the economy, "a rising G.N.P.," and he seeks this not to enrich the commercial classes but "to combat moral pollution." "The technology of economics seems to have been," Reisman writes, "the fashioning of a new world which a philosopher can behold with pleasure" (p. 105). The change to this new world will be smooth and tranquil, in this account, because the basic structural changes it requires have already been determined by basic economic transformations: now what is needed is to secure the balance of powers within government and among classes, so that public power will no longer impede economic growth. The condition of the laboring class and of social morale in general require the bouyancy provided by the progressive state of the economy.

I found the closing portion of the book, where this argument is developed, more authoritative in its use of sources than the earlier parts. Still, the author seems torn between appreciation for Smith's social-mindedness and disdain for what he terms his social conservatism. Sometimes this drives him to serious error, as when he remarks, absurdly, "The *Machtapparat* of the state becomes necessary to police the 'sacred rights of property' " (p. 181). To render Smith's legalistic conception of government in this alien concept doesn't speak well for the quality of Reisman's encounter with Smith's political theory. The work is marred by its failure to render the texts as structured arguments and to relate materials cited or emphasized to these contexts. The author arrogates to himself the right to transmute what he finds into a pattern of reasonings constituting the sort of comprehensive theory he thinks Smith must have had.

In short, the book has some value for scholars following the contemporary discussion about Smith. But it is not sufficiently learned or precise to serve well as a treatment of the issues for those whose interests are less specialized. Contemporary political theorists do well to seek out the theoretical resources hidden in the Scottish moral philosophy, if only to counter the trend toward recasting contemporary issues in sometimes mystifying language conditioned by continental problem constellations. Reisman's book falls short in this respect: he offers *Gattungswesen* to explicate Smith's "universal nature," *Angst* to render fear, as well as *Verstehen* and *Vernunft* and *Geist;* he finds "Panglossian optimism" and "quasi-Mannheimian struggle for the minds of man" and proto-Keynsian situations," and even a "Shakespearean distrust of the masses." Some of these are doubtless stylistic excrecences which his editor should have removed; but one of the things we can learn from Smith's teachings as from his literary practice is that there are vital inner connections between the form and the matter of theoretical work.

DAVID KETTLER

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Freedom and Independence: A Study of the Political Ideas of Hegel's "Phenomenology of Mind." By Judith N. Shklar. (Cambridge, England: Cambridge University Press, 1976. Pp. xv + 216. $14.95.)

In imaginative power and beauty Hegel's *The Phenomenology of Mind* has few equals in the philosophical literature of the world. Yet it is very hard. J. N. Findlay, in his fine book, *Hegel: A Re-examination* (1958), calls it probably "the most difficult of all philosophical works." Its greatness is barely accessible. For it to produce its liberating effects, it must be read in the right mood, with patience, and not too slowly. At the same time, more than most philosophical texts, it seems to beg for the wise commentator. The most studious readers will find themselves lost from time to time, lost in the opaque language, in the intricate scheme, in the general foreignness of the enterprise. We are fortunate to have, therefore, a number of excellent contemporary books devoted primarily to the *Phenomenology* (among them is Findlay's). To that number is now added Judith Shklar's *Freedom and Independence*, a book of surpassing lucidity and strength of mind.

By concentrating on the political themes of the *Phenomenology*, Shklar has provided a coherent account of Hegel's general purpose. This account is no mere summary but a deliberate interpretation. She is determined not only to help her readers (undergraduate and graduate students, in the first instance) see what Hegel is doing, but also to make Hegel yield a sense of life that may speak directly and contrarily to the sense of life Shklar finds too strong in our time. She uses the *Phenomenology* to chasten the young for their love of individual *independence*, of the subjective life whether it takes the form of getting too much pleasure from watching one's inner life or acting in the public realm from the private promptings of conscience. Shklar's Hegel is the resolute champion of public *freedom*, of citizenship in the city-state, the ethos of Periclean Athens. In writing as she does, Shklar, quite in the spirit of Hegel, takes great risks. That is, her interpretation may be found *too* deliberate.

If I read the *Phenomenology* right, Hegel's view is that there are three great manifestations of the human mind: empirical knowledge, moral response, and religion (in order of increasing importance?). All three manifestations result from the same intrinsic human drive: to make sense of the world and thus be reconciled to it. Each manifestation has both conscious and self-conscious forms. Progress is measured by the ascent from the conscious forms (intuitive, untutored) to the self-conscious forms (in them, the mind has an intimation of itself as a seeker of meaning in the world, and thus not only acquires better empirical knowledge, moral response, and religion, but also has some awareness that these three manifestations are not mere outwardly induced reflexes but rather efforts of the mind to make sense of the world and thus be reconciled to and with it). The self-conscious forms of each have, and coexist with, corresponding self-conscious forms of the other two. All self-conscious forms necessarily change because the human mind is incapable of resting in satisfaction with any proffered reconciliation that is known not to be fully truthful. They have all eventually been found deficient. Their changes follow roughly the same pattern of ever greater degrees of complexity of self-consciousness, but at any given time, they are as truthful as they can be. No form, conscious or self-conscious, is complete error, or is dispensable in the ascent of self-consciousness, or is ever totally obliterated. The goal of the human mind, now reached, is precisely to learn these facts about the forms of the past, and, by using these forms and going beyond them, to arrive at the true sense of the world and thus to true reconciliation. The truth could be found only arduously, even though it was never hidden. The meaning, toward which all the forms have been straining is that (in empirical knowledge) the processes of natural change are structurally similar to the processes of the mind's growth; that (in moral response) ego and human other are the same, are equal; and that (in religion) man and God are the same. The discovery of resemblance is the basis of reconciliation. The equivalencies cut both ways. In the light of absolute knowledge: nature is humanized and man naturalized; the ego gains the other by losing itself to it; God gains real (self-conscious) existence when humans (as completed self-consciousness) assert their own divinity. The world is mind. Hegel does not foresee rest even with the acquisition of the true sense: on the last two pages of his book, he sets the human mind off again, but now it begins its course "at a higher level." Thus, if Hegel was, as Shklar says, hostile to the claims of any individual's subjective independence, he was also the great destroyer of the claims made for the independence of objective reality from human consciousness. He was, in truth, a phenomenologist.

Out of this extraordinary conception Shklar has focused on moral response, the manifestation to which Hegel gives the greatest amount of attention, by far, in the *Phenomenology*. She divides her book into five chapters. The first gives an overall "topography" of the *Phenomenology* and is a splendid act of compression without simplification. She detects five cycles of forms: one (the basic one) for empirical knowledge; three (not in all respects temporally consecutive) for moral response; and one for religion. The four subsequent chapters deal mostly with the three cycles through which Hegel sees the human mind go in having, and then trying to make sense of, moral experience. At the end of the last chapter Shklar takes up the cycle of religion. On the *Phenomenology* as a whole, as well as on many of its particular arguments and reconstructions, she throws a glorious light. You have to read the *Phenomenology* to know how good Shklar's book is; and if you read Shklar's book you will want to read the *Phenomenology.*

To praise the book is not to agree with everything in it. I think that Shklar may overemphasize Hegel's attachment to Athens. After all, he makes clear that the Athenian harmony of self and society left out too much possible and necessary human experience, was too far away from a truthful reconciliation to and with the world. The most sublime moment in the *Phenomenology* is the climax of the third moral cycle: the confession by the conscientious political actor of his impurity and the reluctant acceptance by the hard-hearted "beautiful soul" that his severe judgments of the actor, made in the safety of inaction, have been blind to the essential commonality between actor and judge. The judge says Yes to the actor. With that Yes, Hegel says, God appears. I assume Hegel did not think that Athens would have contained the experience to yield that truth. Christianity alone could create the subjectivity which the full truth needs. Shklar is aware of this moment (p. 196), but I think she is too eager to dissolve it in the later theory of *The Philosophy of Right.* It should not be dissolved because it is symptomatic of Hegel's entire outlook on the world, as presented in the *Phenomenology.*

GEORGE KATEB

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From Georges Sorel: Essays in Socialism and Philosophy. Edited with an Introduction by John L. Stanley. Translated by John and Charlotte Stanley. (New York: Oxford University Press, 1976. Pp. 388. $15.00, cloth; $5.00, paper.)

In an age of both academic and linguistic insularity, the fame and fortune of scholars often follows rather than precedes translation efforts, although with our cosmopolitan pretensions we would like to assume the contrary. Without the pioneering efforts of George Simpson and Joseph Swain on behalf of Durkheim, Talcott Parsons' and Frank Knight's work on Weber, and T. E. Hulme's translation of Sorel, I seriously doubt that these giants of European thought would be accorded the fame that later properly accrued to them in the Anglo-American world of letters. Once the floodgates of criticism and adulation are unleashed, more translations follow. But those first pioneering efforts can hardly be minimized as crucial links in the transmission of ideas. Lest we bemoan the loss of scholarship in the present age, it is only fair to add that with the supreme efforts of William W. Holdheim (on Scheler); Gunther Roth (on Weber); H. S. Harris (on Gentile); Rodney Livingston (on Marx); Everett K. Wilson (on Durkheim), among others, the younger generation has more than kept faith with the needs of translating social science classics. Indeed, they have done so in a far more systematic and capable manner than past researchers dared imagine.

A special niche is occupied by John L. Stanley, who along with his wife Charlotte, have not only been involved in the critical analysis of the work of Georges Sorel, but also previously translated *Les illusions du progrès.* When one thinks that Sorel's entire reputation in English rested almost exclusively on the translation of *Réflexions sur la violence,* the impact of that great French scholar can be properly gauged. Other than my own translation of *La décomposition du marxisme,* I do not know of any other major work by Sorel up to the time of the work of the Stanleys, that has been made available in English. Thus, one must applaud their nobility of effort as well as their real achievement in this unprepossessing area of scholarship.

At the same time, a certain note of appreciation should be made in the direction of Oxford University Press and its imaginative editorial staff for having developed a most unusual "greats" series. Starting with *From Max Weber,* extending through *From Karl Mannheim,* they have now added *From Georges Sorel.* Precisely because the company is select, the idea is sound. One can only hope that Oxford will continue to produce a series that will both rival and supplement the University of Chicago's outstanding Heritage of Sociology Series headed up by Professor Morris Janowitz. What this adds up to is that the history of political ideas, along with the sociology of knowledge—I am beginning to wonder if there is much difference—is flourishing and healthy.

The translations themselves, albeit of selected fragments, are competently done, or so it would seem from my own recollection of working with Sorel documents. But I do feel that Mr. Stanley's literalist interpretations of key words may impede rather than enhance the reader's sense of context as well as contents. Three title headings illustrations will suffice. *L'avenir socialiste des syndicats* is translated as "The Socialist Future of the Syndicates." My own understanding of the contents would lead me to translate this as "The Future of Trade Unions Under Socialism." *Matériaux d'une théorie du prolétariat* is rendered as "Materials for a Theory of the Proletariat," when in fact something like "Elements of" or "Essays toward" a Theory of the Proletariat seems less ambiguous. Finally, *De l'utilité du pragmatisme* would be better understood as "The Uses of Pragmatism," since in English, utility and pragmatism are somewhat tautological, or at least kindred concepts. But these are minor matters of preference. And when translations are done by team effort, as these were, then the tendency to stick to the literal becomes overpowering and quite understandable.

A somewhat disappointing aspect of this collection is the paucity of materials indicating the extraordinary range of Sorel's mind, from the development of modern physics to the origins of religious systems. He is one of the few peripatetic figures we have had, and probably unequalled in imagination and intellectual power until Harold Lasswell arrived on the American scene. Regrettably, there are no selections from the very late as well as very early periods of Sorel's thought. One selection from the 1919–1922 period, when Sorel came to grips with the mortality of his self and the immortality of the souls of others, might have been a useful rounding out of the Introduction. There is too little of Sorel as an analyst of other theorists. Especially appealing might have been some of his earlier essays from *Le Devenir Social* on Renan, Durkheim, James, Vico, Peguy, and Bergson, among others. As Stanley's introduction makes plain, Sorel was at his best as critic. He was more penetrating as an analyst of the foibles and fables of others than he was a systematic theorist. The tendency in any anthrology or compendium is to assume that selections somehow are drawn from a systematic body, when in fact Sorel was a consummate essayist.

Stanley's own introductory essay and bibliographic appendix is a model of sound scholarship. The analysis is carefully drawn, clear and correct in its details and general outline. It is written in an entirely appealing graceful style well suited to the general audience this book hopes to reach. Stanley knows Sorel's work; he knows the literature on Sorel; and he avoids the usual academic pitfall of ignoring one at the expense of the other. If the events in France that moved Sorel from Marxism to fascism to communism are cloudy, that is because the European intellectual's sense of how they differ did not have the benefit of historical hindsight.

Because of the limited amount of material available on Sorel, one would have hoped that this might have been a more extensive set of papers. But failing that, it might have been wise to simply indicate the availability of *Les illusions du progrès* and *Réflexions sur la violence* in other places, and hence exclude selections from these works already easily available, while concentrating on other essays less easily available. On the other hand, the need to offer a balanced, complete picture made Professor Stanley's decision entirely understandable. One can only hope that these translated essays are but a prelude to a larger effort by Stanley to give us a translation, in systematic fashion, of the corpus of Sorel's works. There is a great need for translations of his basic writings in full. In particular: *Materiaux d'une théorie du proletariat, Introduction à l'économie moderne,* and of *De l'utilité du pragmatisme.* An excellent reason for having Professor Stanley serve as translator, or at least chief supervisor of this project, is his fluent knowledgeability of the Sorelian literature, his sense of both the historical and intellectual setting of this great thinker. It is high irony that as we move toward the close of the twentieth century, and hence another *fin de siècle,* once again we witness the cry of linking socialism and Christianity, class and nation, political action and moral virtue, new linkages of social action to political theory. In this resurrection of normative themes we can hardly do better than to encourage the long overdue exacting effort at translating the works of Sorel. For he was after all a classicist in outlook as well as part of the classical tradition. In the meanwhile, *From Georges Sorel* is an excellent starting point for those not acquainted with this extraordinary thinker. All of us concerned with at least how one political analyst's past informs the present owe a real debt of gratitude to John Stanley.

IRVING LOUIS HOROWITZ

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Anarchy and Cooperation. By Michael Taylor. (New York: John Wiley & Sons, 1976. Pp. viii + 151. $13.95.)

Taylor's book is a study of the possibility (and rationality) of voluntary cooperation among individuals when binding agreements to insure cooperation are not possible. The first several chapters contain an analysis of the prisoners' dilemma supergame (an infinite sequence of ordinary prisoners' dilemma games), and present results showing that cooperation in this game can indeed be rational under certain conditions. A subsequent chapter contains a detailed discussion of the parallels between the prisoners' dilemma supergame and the analysis of behavior in the state of nature offered by Hobbes and Hume. Taylor argues that his results concerning prisoners' dilemma supergames provide an important criticism of the primary justification of the state found in the contractarian literature, namely, that in the absence of the state cooperation among individuals is impossible. The book is noteworthy in two respects. Taylor's analysis of the prisoners' dilemma supergame is an important addition to the game theory literature and deserves the attention of anyone interested in this fascinating game. In addition, the care with which Taylor discusses the relationship between the prisoners' dilemma and the arguments of Hobbes and Hume sets new standards for mathematical theorists who seek to formalize the arguments of classical political philosophers.

Chapter 2 discusses the two major models of "free rider" behavior, the prisoners' dilemma and the model derived from the theory of public goods. One important effect of using a binary choice prisoners' dilemma to model behavior in the state of nature is that it removes all ambiguity from the determination of a cooperative individual act and a cooperative collective outcome. In the public goods model, there are infinite sets of individually cooperative acts (given by an individual's demand curve for the public good) and collectively cooperative outcomes (the set of Pareto optimal allocations). To endow these actions and outcomes with the prominence (in Schelling's sense) given them by the binary choice, prisoners' dilemma places major constraints on the ability of the game-theoretic analysis to encompass many of the subtler aspects of behavior in the state of nature which are central to the works of Hobbes and Hume.

Chapter 3 contains the heart of the mathematical theory. The two-person prisoners' dilemma supergame is analyzed and the results are then partially extended to the N-person case. A supergame is an infinite sequence of identical ordinary prisoners' dilemma games in which each player must choose between cooperating or defecting. Players are assumed to evaluate sequences of payoffs by discounting them exponentially. A strategy in the supergame is an infinite sequence of choices, each of which may be conditional on outcomes of all previous ordinary games. Taylor's analysis focuses on five important types of strategies: C (always cooperate), D (always defect), $A_{k,n}$ (cooperate in the first game, and continue cooperating as long as at least n others cooperate. If fewer than n others cooperate, defect in the next k games, then return to cooperation and begin again), B_n (cooperate in the first game, and from then on cooperate if at least n others cooperated in the previous game), and B'_n (defect in the first game and from then on cooperate if at least n others cooperated in the previous game). The $A_{k,n}$ strategies punish defectors but always give them a chance to redeem themselves by cooperating in a later game. The B_n and B'_n strategies have been called "tit-for-tat" strategies in the literature. Taylor demonstrates, for the two-person game, that strategy pairs calling for conditional cooperation can be in equilibrium, and thus that cooperation among individuals in a state of nature (i.e., able to communicate but unable to reach binding agreements) is indeed possible. The conditions which must be met have strong intuitive appeal: the greater the temptation to defect (as measured by the entries of the payoff matrix) and the greater the rate used to discount payoffs, the less the chance of a cooperative outcome. In the N-person game, an additional (and important) condition must be met: the cooperation of all conditional cooperators must depend upon the cooperation of all players not using strategy D. An important omission in Taylor's analysis of the N-person game is his failure to establish the Pareto optimality of those equilibria with less than complete cooperation. If these equilibria are not Pareto optimal, Taylor has not countered the contractarian argument to the extent he believes.

Chapter 4 extends the earlier analysis to the case in which an individual's preferences include some degree of altruism (either positive or negative). Such an extension helps to move the mathematical theory beyond the egoistic preferences so much objected to by most political theorists. In particular, in discussing Hobbes' assumption that individuals attempt to maximize their "eminence," Taylor argues that this is accurately modeled by assuming that an individual attempts to maximize a weighted combination of his/her own payoff and the average advantage he/she enjoys relative to his/her peers.

Chapter 5 provides a non-mathematical summary of the analysis in the previous two chapters. Its inclusion enables readers who are so inclined to bypass Taylor's detailed mathematical arguments and to move on to more quickly to his discussion of Hobbes and Hume.

Chapter 6 is devoted to a detailed discussion of the assumptions underlying behavior in the state of nature in the analyses of Hobbes and Hume, and the extent to which this behavior can be accurately represented by an N-person prisoners' dilemma supergame. This chapter represents, to my knowledge, the most serious attempt by a mathematical political theorist to relate in great detail the inner workings of a mathematical model to the non-mathematical inner workings of the arguments of classical political philosophers.

The final chapter concerns the dynamic nature of human preferences and motivations, and criticizes static models of human behavior in which individuals are endowed with preferences which remain constant. Drawing on the anarchist literature, Taylor argues that neither contractarian nor game theorists deal adequately wit the effects of the state on individual preferences or the changes in preferences which might occur in the absence of the state.

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Bureaucracy and the Modern World. By Victor A. Thompson. (Morristown, N.J.: General Learning Press, 1976. Pp. 141. $5.95 paper.)

This book contains a set of five essays by the author, three of which were previously published as "modules" by General Learning Press, and one which represents a condensed version of his more extended essay, *Without Sympathy or Enthusiasm: The Problem of Administrative Compassion* (University of Alabama Press, 1975). As Thompson notes in his Preface, "the book contains a strongly argued point of view," although it would probably be more accurate to say that each essay advances its own particular theme or thesis in strongly argued terms. Students of administration should have no difficulty in recognizing the bureaucratic model which Thompson extols. Without equivocation or qualification, Thompson's interwoven pattern of bureaucracy in the modern world is an intricately designed reflection of the seminal theories of Max Weber, Talcott Parsons, Herbert Simon, and Charles Lindblom. The net result is bureaucratic organization characterized in the context of instrumental, reductionist, universalistic, and impersonal terms.

In his first essay, "Organizations as Systems," Thompson attempts to explain the Janus-faced character of all organizations in terms of a constant interaction and integration between the instrumental rationality of the organization as a closed, artificial system, and the intrinsic rationality of the individual members of the organization who compose an open, natural system. The ultimate value criterion of the organization as a closed, artificial system of rational calculation is, of course, efficiency, while for the natural, open system of spontaneous, human interaction the criterion of success is survival. Thompson's system dichotomy is completed, as might be anticipated, by designating the principal actors of each system as "economic man versus organization man." Thompson's second essay, "The Development of Modern Bureaucracy," represents a detailed and elaborate description of the artificial, mechanistic aspects of organizational structure (hierarchy, span of control, chain of command) and the manner in which these structural characteristics are energized and utilized by individual human beings who are engaged in a perpetual struggle of power for control. Thus, the natural system is in constant conflict with the artificial system; survival is constantly being pitted against efficiency. This conflict can never be eliminated but it can be minimized through the intelligent application of the incremental decision-making model as advanced by Lindblom. This is the "radical" proposal advanced by Thompson in his third essay, "Decision Theory, Pure and Applied," in which he also advances the standard criticism of the rational comprehensive decision-making model and its applied effort, PPB. The fourth essay, "Bureaucracy and Innovative Action," examines the concepts of innovation, change, and creativity within the context of bureaucratic unity, rationality, hierarchy, and specialization, and concludes that bureaucracies are not innovative because "they are not supposed to be innovative." In other words, individuals may be creative and innovative but organizations—those artificial systems that are simply tools to be utilized—should be primarily viewed as devices for maintaining a steady, "business as usual" routine. Essentially the same logic may be applied to describe his final essay, "Bureaucracy and Compassion." Modern large scale bureaucracies are not compassionate because they are not supposed to be compassionate because they cannot afford to be compassionate. Thompson provides a strongly worded response to the New Public Administration-organizational humanism school, and actually, his more extended monograph referred to above, from which this essay has been derived, should be read by those interested in examining the full scope of the argument he advances.

Of the five essays in this book, the first three do provide a logical continuity, and could be profitably utilized as a single unit for instructional purposes. The fourth essay, as far as I can determine, is simply a "filler"; although it was obviously written quite some time after the first essay, the natural-artificial systems described in rigorous detail in the first are reintroduced *de-novo* in the fourth essay. In addition to this essay's redundancy, it is also the least well-written of the five. Thompson is at his best, I think, when he writes as an analytical theorist; his first three essays are tightly constructed, well-organized efforts, and stand in direct contrast to the fourth essay which reads in an extremely rambling and discordant fashion. Moreover, the argument that bureaucratic organizations are not innovative because they are not supposed to be innovative (p. 101) will create something of a problem for many readers who will recall that, in extolling the virtues of incrementalism, Thompson notes "incrementalism makes creativity and innovation possible. . . . The inertia of large organizations arises not out of incremental strategy but out of the attempt to be comprehensive and the kind of administrative organization that this attempt promotes" (p. 68). His critique of PPB, while undoubtedly quite relevant when originally written, is stale and dated if not outdated given the recent developments of the Congressional Budget Office and such recent administrative efforts as MBO—management by objectives.

Thompson is an interesting writer; he charts his course carefully between what he considers to be the metaphysical abstraction of the artificial system—i.e., organizational goals which assume a collective or group mind (p. 26)—and the metaphysical, if not mystical, mystery which shrouds the natural system with the darkness of forbidden knowledge (p. 22). The net result of his efforts is a strong reaffirmation of the pluralist-bargaining-incremental system. Thompson argues, in effect, that the PBI system is the most preferable for the present and the future mainly because it is the least disruptive of the past. The myths of the id, as well as the myths of the machine are, I am convinced, the prime causes for the perpetuation of the myths of the virtues of incrementalism. For those who agree with Thompson that incrementalism is a "radical proposal" for simplifying the task of making rational decisions (p. 66), then moderation in pursuit of freedom is indeed a prime virtue. *Bureaucracy and the Modern World* is a re-declaration of an ethic that has not served us well. Thompson argues his case forcefully, but the credibility of the value base on which he stands—the pluralist-bargaining-incremental system—has eroded.

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Tolerance: Toward a New Civility. By Glenn Tinder. (Amherst, Mass.: University of Massachusetts Press, 1976. Pp. 187. $12.50.)

This book is at once a full-scale revision of the case for liberty of expression, as set forth by Locke and Mill, and a reply to those modern radicals who believe that free speech seldom brings to light the most serious injustices of our society and that liberalism's tolerance tends actually to lull us into unawareness or disregard for those injustices. It is the most extensive and comprehensive restatement of the liberal position in non-"liberal" terms known to this reviewer.

The fundamental assumptions underlying the case for liberty of expression, according to Tinder, are those of (1) individualism, (2) rationalism, and (3) historical optimism. He attacks them each in turn. Thus he argues that "neither Locke nor Mill builds effective defenses against the assumption that tolerance may eventuate in doubt and confusion and that truth may be discovered under conditions of restricted freedom" (pp. 30–31). The individualism of classical liberalism has tended to divert attention from people's social relations and to support "complacent acquiescence in injustice" (p. 51). (Did it with Mill?) The optimistic belief in progress encouraged that tendency. Hence Tinder sets out to inquire whether tolerance is defensible on a set of postulates opposed to those of Locke and Mill.

Tinder's rival principles are those of "communalism," "transrationalism," and "historical pessimism." Community, as he defines the term, "the unity of men in their essence" (p. 60), depends upon communication. Moreover, the communication must be honest and responsible. Thus "the substance of community is not the all-engulfing emotional or habitual unity that drowns all personal distinctions and all thought but, rather, communication" (p. 70). Tolerance is a necessary although not a sufficient condition for community, because community entails communication, which depends upon tolerance. Community depends upon engagement, rather than, as with Mill, upon disengagement.

Truth, like community, depends upon tolerance. The argument here is superficially like that of Mill, but Tinder stresses truth about matters that are not rationally demonstrable (hence "transrationalism"), about such central insights as man's essential equality. His position is, as he puts it, relativist with respect to evaluations but not with respect to all values, the value of community, in particular, being in his view absolute.

As to "historical pessimism," Tinder has little difficulty in demonstrating that it, as well as liberalism's optimism, calls for tolerance, although a different kind of tolerance from that of liberalism. It is receptive rather than active or utilitarian, realistic rather than confident. The significance of the distinction and the fact that it is directed more against today's radicals than against nineteenth-century liberals will be gleaned from the following passage. "Tolerance," he writes, "does not necessarily mean acceptance of injustice but only an unwillingness to initiate an attack on it by abandoning receptivity. A tolerant radicalism might refuse to be involved in any reformist or revolutionary activity that seriously imperils possibilities of communication" (p. 125).

In the latter part of the volume the argument assumes an Existentialist and indeed a religious tone. Tolerance is a burden, one that is imposed upon us by uncertainty and human unreliability, not least the unreliability and the finitude of oneself. Tolerance in the final analysis must depend on something that stretches beyond fallible man—a sense of eternity.

Tinder's arguments differ from those of classical liberalism less in their conclusions and practical applications than in the audience to which they are addressed and the values to which they appeal, yet differences in application do appear. His theory is more flexible, less absolutistic in its prescriptions than those of Locke and Mill. The characteristics of the tolerant ("attentive and open") society must embody order, unity, spiritual transparency, and (shades of Mill!) "the spirit of deference." Where does this lead? While in general it leads to freedom, it also leads to the principle "that we are justified in being intolerant of all that destroys tolerance, and of all that destroys the conditions rendering tolerance productive of community and truth. . ." (p. 158). Thus not only violence but also the glamorization of violence may be barred. Also obscenity, because it depersonalizes and, in doing so, eradicates "the mutual respect on which a communal tolerance must depend" (p. 159). Tinder stops short, however, of justifying censorship of the media, substituting the use of government subsidies to destroy the monopoly of the commercial networks and the advertisers. This tantalyzing suggestion is not followed up with any details about how it might be implemented. Would commercial television and radio be allowed to exist? If so, would the problem be solved, or even ameliorated? If not, what happens to the open society and freedom of communication?

Arguments (by geneticists) for the genetic inferiority of certain races, upsetting though they are, must be tolerated, for they address themselves to reason. Moreover, acts of a serious nature, communicative in intent and effect, such as desecration of the flag, must be permitted. So also with any communicative attempts to call into question the legitimacy of the democratic regime. But that rock of liberal philosophy, the "clear-and-present-danger" test for acts of governmental self-defense, is accepted.

Community is not at odds with pluralism. In fact, Tinder contends, "community is the positive goal of pluralism" (p. 170), thus reminding us that his definition of community is Aristotelian rather than Platonic. It "is not harmonious order but fruitful conflict" (p. 175).

Many of Tinder's criticisms of the classical liberals are, if not unfair, at least overdrawn. And frequently his own arguments, notwithstanding his inversion of Mill's basic principles, seem less unlike Mill's position than Tinder would lead one to expect. Perhaps, after all, the most significant fact that derives from this book is that a case for liberty of expression and communication not unlike Mill's follows from principles that are sharply opposed to those of classical liberalism. When like conclusions derive from opposing principles, the conclusions are strengthened; and one begins to suspect that they ultimately derive from something more fundamental than either set of principles, such as a belief in human equality. Mill's alleged "rationalism" and Tinder's "transrationalism" are less sharply opposed than Tinder makes them out to be, and the same can be said of Mill's "individualism" and Tinder's "communalism." And, as for "optimism" and "pessimism," while they are clearly enough mutually opposed, it is by no means that either is essential to the case for tolerance or, for that matter, incompatible with it.

Tinder's volume is thought-provocative and suggestive as well as clarifying with reference to the tolerance-doubting ideas of many modern radicals. Anyone concerned with tolerance today should know his Tinder as well as his Locke and Mill.

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Law and Politics in the Middle Ages. By Walter Ullmann. (Ithaca, N.Y.: Cornell University Press, 1975. Pp. 320. $15.00.)

Both the author and the editor of the series in which this book appears view it as an introduction to the sources of political thought in the Middle Ages. A more accurate description would be that it is an introduction to the sources of western political thought written in Latin or in western vernaculars during the Middle Ages. With one exception, a crude and biased assertion that defies the historical record, Ullmann has entirely neglected the medieval Arabic and Hebrew contributions to the development of western political thought. This criticism notwithstanding, Ullmann's mastery of his subject is truly impressive. He knows most of the sources quite well and has an exceptional knowledge of the vast body of secondary literature germane to those sources.

Ullmann has divided the book into eight chapters. An introductory statement about the relationship between law and political theory as well as that between Roman law and Christianity is followed by five chapters on the various kinds of law dominating the western Middle Ages and the kinds of scholarship arising in response to each kind of law. In the last two chapters, he discusses various non-legal sources of political thought and gives a brief overview of the political thought that grew out of the renewed interest in Aristotle during the later Middle Ages. The major emphasis in the book is on the legal sources, for Ullmann is persuaded that almost all political ideas arose from the study of the law until the Aristotelian revival of the thirteenth century. Since the major purpose of the book is to describe the legal as well as the non-legal sources of medieval political ideas, it has no basic argument. Only in the introductory chapter and in the general remarks made by the author in the presentation of the various sources does anything resembling an argument appear.

According to Ullmann, Christianity absorbed the Roman idea of law at a very early stage and then tried to influence Roman law with Christian doctrines. In addition to helping the Church become legitimate, this fusion prepared the way for the subsequent substitution of the Bible for the ancient Roman constitution and for the eventual modification of secular law by canon law scholars. Throughout his account of the Roman, canon, and folk law sources Ullmann refers to the persistent tensions which existed between the secular and ecclesiastic representatives and suggests that the major victory of the Church was its success in insinuating itself into secular matters. However, he never provides a clear and coherent description of this maneuvering. Generally speaking, his whole treatment of the historical background is spotty and uneven: some issues are set forth in excruciating detail or in simplistic fashion, while others are passed over rapidly with a one-word reference or a brief footnote about the relevant literature. (In this respect, the author's own expertise betrays him: he refers to his own books and articles so frequently that he fails to let this work stand on its own merits.) This is all the more unfortunate as the book is self-consciously historicist. Ullmann is firmly persuaded that "governmental actions and political ideas cannot be considered in a vacuum" and that he must "show the kinds and the growth of the sources in their relation to the historical background." Yet the only way in which his historicism is consistently present is in his incessant identification of political ideas or political theory as ideology, an identification which he never defends and which contributes nothing to the theme of the book.

In his discussion of Roman and canon law, Ullmann first explains each kind of law and identifies its sources; then he enumerates the different kinds of scholarly works concerned with each kind of law. These chapters are very informative and extremely rich in basic references, but so poorly organized that it is sometimes almost impossible to follow the thread of the exposition. In the last chapter, he tries to show how the Aristotelian revival at the beginning of the thirteenth century led to massive changes in the general climate of political thought and he identifies two basic ideas as representative of the new revolution: the change from the "wholeness" view that everything was related to or subsumed under Christian doctrine to a view that permitted human actions to be divided into different categories and the introduction of nature as a standard of judgment. While that account is accurate as far as it goes, Ullmann's actual references to Aristotle suggest a misreading of the texts: he completely ignores Aristotle's insistence on the good life as the reason for which the city comes into being and he presents

Aristotle's distinction between the good man and the citizen as proof that "man and citizen corresponded to two different categories of thought."

In sum, this is a book that must be used with great care. In addition to the substantive problems already alluded to, the general exposition of the book is gravely marred by a number of passages that defy the normal rules of English construction and by an abundance of typographical errors. Nonetheless, as a one-volume reference to myriad sources in medieval western political thought and to the basic secondary literature about some of these sources, it is extremely valuable. For that, we must be thankful.

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Knowledge and Politics. By Roberto Mangabeira Unger. (New York: Free Press, 1975. Pp. ix + 336. $12.95.)

Announcements of the crisis, poverty, or even the end of liberalism have become sufficiently regular so as to blunt any expectation that we are moving beyond liberalism. Roberto Mangabeira Unger's *Knowledge and Politics* proves the point. His work is a wide-ranging and unusually intensive critique of liberalism; but it fails to mark out a new position more viable than liberalism.

This book is offered as a "total criticism" of liberalism. While partial criticisms reject this or that aspect of liberalism, as the specialized interests of the critic require, Unger aims to show that a rejection of any part requires the discarding of the whole. In order to make good on this claim, the author must maneuver liberalism onto a very high plane of abstraction. It is not the liberal doctrine of obligation, nor the liberal theory of contract, nor any other particular liberal teaching that is examined. Rather, it is the characteristic understandings and assumptions behind all liberal doctrines that are to be exposed. The reader, therefore, needs to be warned (informed) that he will not find a discussion of any particular liberal writer in this book. Nor does the book distinguish among liberalisms. Rather, liberalism is treated as a "metaphysical conception of mind and society" (p. 6). What gives the study its title is the insistence that a few key metaphysical postulates tie together liberal ideas of the self, politics, ethnics, law, and so forth. By far the most intriguing and careful part of the work is its reconstruction and criticism of those postulates.

At the center of the liberal metaphysic, we are told, is a particular and faulty handling of the relationship between "the universal and the particular." By rejecting the classical doctrine of intelligible essences and by affirming the subjectivity of values, the liberal, it is argued, commits himself to a separation of the universal and the particular that appears as the divisions between theory and fact, reason and desire, and rules and values. These divisions entangle liberalism in a series of antinomies that mar the doctrine and expose the liberal experience as immoral.

The easiest place to examine Unger's criticism of the liberal metaphysic is in his discussion of the liberal principle of reason and desire. This principle, he argues, separates the understanding of a fact and the choice of a goal. Reason, man's universal quality, is confined to the world of means; the only rationality is instrumental rationality. The ends and substance of life are particular and, thus, private. Such a theory is destructive of any account of a unified self and prohibits that sort of shared commitment to universal ends that gives moral significance to community life. A morality faithful to the principle of desire is nonsensical, since the principle provides no basis for preferring some desires over others. This side of liberalism collapses values into subjective choices. A morality faithful to the principle of reason initially seems more promising. Reason can set down certain coherent standards or rules. But in order to make the commands and prohibitions of reason concrete, the liberal would have to judge among conflicting individual goals; and it is just this that, according to the principle of desire, reason lacks authority to do. The separation of reason and desire means that the rules which are to serve as the foundation of order and freedom in liberal society will facillate between indeterminancy and *ad hoc* decisions. In short, one of the major antinomies of liberalism is produced by the fact that it both must and cannot assign a prescriptive role to reason.

Tugging on the same threat, Unger argues that the liberal treatment of reason and desire has its political analogue in the liberal theory of legislation and adjudication. The principle of desire explains why the Hobbesian problem of how people can live together dominates liberal political theory. The liberal emphasis on the role of constitutions, rules, and laws in society is dictated by its need for some method to keep the peace. But the denial of objective standards means that liberalism again confronts a conundrum of its own making. The liberal state is supposed to be neutral so as to remain free of the charge that it is giving preference to some over others. But the very doctrine of subjectivity of values that supports the demand for neutrality also deprives liberalism of any justifiable criteria for "defining general categories of acts and persons when we make the rules" (p. 80). Nor can liberalism provide any criteria for arranging and balancing the possible rules applicable in a case. The author concludes that no coherent doctrine of legislation or adjudication is possible on liberal premises. Another chapter has an interesting and useful discussion of the welfare state which examines the differences between a class society and a society organized by role structures. Unger's thesis is that the welfare state is marked by the conflict between the ideal of organization according to impersonal roles and the reality of personal dependency produced by those roles. The dualisms of early liberalism persist in the welfare state.

The last chapters explore the theory and conditions necessary for the realization of a post-liberal society. At this point, the book's weaknesses become most apparent. The account of organic groups as the way to realize the ideals of "natural harmony, sympathy, and concrete universality" avoids any reference either to example, history, or observation. The best we are offered here is an invitation to "look then into yourself" to test the validity of the author's claims. The "good" is defined as the realization of universal human nature as revealed over time in a fully democratic society composed of organic groups. But the best the author can do in answering the criticism that human nature is, at best, morally indifferent is to suggest that such a belief, if true, would plunge us back into the world of subjective values. This is no longer an argument; it is an act of faith. As such, it is no more substantial than the complaint of Thomas Reid that if what Hume said about human nature is true, he preferred not to know it.

While this is an important work, its weaknesses are considerable. There is no willingness to take account of the developments in moral philosophy that have moved beyond the quarrel between absolutism and relativism. Also, Unger's total criticism of liberalism is purchased at a very high price. The differences between, say, Hobbes, Locke, and even Hume must be treated as a family quarrel. Unger's definition of liberalism is so abstract that it becomes all-inclusive and hampers our ability to distinguish among very different doctrines.

ALFONSO J. DAMICO

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American Politics

Unequal Justice: Lawyers and Social Change in Modern America. By Jerold S. Auerbach. (New York: Oxford University Press, 1976. Pp. xiii + 395. $13.95.)

This is a first-class book: a richly detailed, impressively analyzed, and lucidly written account of the first century of the organized Bar in the United States. It is not a conventional "balanced" history, but a powerful indictment of the failure of the legal profession to assume social responsibility, of its constituent subservience to the policies of conformity and orthodoxy in the service of clients of affluence, and of the systematic class and ethnic bias which has long infected the organizations of the law and defined the services which lawyers provide. In the absence of serious public regulation, the legal profession's elite pursued a course which maximized private gain, often confusing the pursuit of profit and the service of clients with the public interest. The rhetoric of equal justice under law masked the reality of "unequal justice under lawyers" (p. 308).

These are strong words, and they will not be welcome at the Bar. But at a time when the legitimacy of lawyers is at a low ebb, and the taint of Watergate has not yet worn off, lawyers and law students will have to meet Auerbach's challenge. It is not sufficient merely to make the delivery of conventional legal services more efficient and more widespread. The organized Bar must assume a collective responsibility for the achievement of social justice. Legal "ethics" is more than regulating the relationship between lawyer and client, or establishing rules for courtroom behavior. The consequences of the Bar's failure to perceive this distinction and act accordingly, and not merely the misdeeds of a few malefactors, must be the message of Watergate for lawyers in America.

Auerbach shows how the stratification of the Bar, both as to clients and lawyers, enabled a relatively few, influential, elite lawyers to speak for it and dominate its professional life. Efforts to exclude ethnic minorities included opposition to the development of proprietary law schools, support of "higher" prelaw educational requirements which had a strong class bias effect, political tests for Bar admission such as loyalty oaths, and stricter Bar admission requirements. Those who got through this first net met even more formidable obstacles. Exclusion from the major law firms meant that most ethnics, without substantial economic resources, began as solo practitioners, or in small firms. Business just didn't come their way, and the Canons of Legal Ethics (now the Code of Professional Responsibility) obstructed the search for clients. Under the guise of promoting individual responsibility and protecting citizens from the guile of a few unscrupulous lawyers, the Canons prohibited the solicitation of legal business, strictly limited advertising and contingent fee cases, and established minimum fee schedules to prevent "unprofessional" competition for clients. All of this made it most difficult for solo practitioners to survive economically.

To be sure, the efforts of the elite Bar were not wholly successful. Today, night schools proliferate, ethnic lawyers have made their mark as intellectual leaders in the top law schools, and some have risen to the top of their profession, including the Supreme Court. There has been grudging absorption of minorities into the larger law firms. Moreover, recent studies suggest that elite lawyers were never as interested in enforcing these restrictive Canons of Ethics as they were in proclaiming their own virtue through them. Carlin's *Lawyers' Ethics* makes clear that the greatest concern of the elite Bar of New York City was in maintaining these *symbols* of rectitude (as they defined it).

Forces outside the Bar also had a leavening influence. The New Deal in particular was a watershed in the transition toward somewhat greater democracy in the profession. Elite lawyers did not lose their class consciousness or distaste of upwardly mobile interlopers. But they recognized the skills which ethnic lawyers had acquired in the building of the administrative state, and the need to provide their own clients with such skills. Thus one can share Auerbach's condemnation of the motivations and values of the elite Bar and of its exclusionary policies without accepting entirely his assessment of their influence. The Bar has always been somewhat more complex, and the elite Bar less powerful, than its leaders' statements indicate.

Research of a different kind, yet to be done, is needed to pinpoint exactly how exclusive the Bar really was. Was the percentage of ethnic lawyers significantly lower than the percentage of ethnics in other professions? My own prediction is that such evidence, when unearthed, will show that ethnics flocked to the Bar because it was, on the whole, easier to gain admission there than to many other professions. Lawyers were not demonstrably *more* prejudiced than most Americans, though the consequences of their prejudice had more serious consequences because of the central role of law and legal institutions.

It is also true that some of the Canons of Ethics, whatever the motivation behind them and past misuse, may nonetheless have redeeming validity. For example, most would agree that there ought to be some limitation on solicitation and advertising of lawyers' services. But where should the line be drawn? Can it be drawn in such a way as to protect the interests of both clients and lawyers and also be sensitive to considerations of free speech? Can it be drawn in a manner which does not reinforce existing class or ethnic biases? Removing all such restraints would not necessarily promote Auerbach's larger goal of a more cohesive Bar with a liberal collective conscience.

Auerbach is effective but also not wholly convincing in his condemnation of the "process" or individual service model of legal activity. In theory, by selling their services, and not their souls, lawyers conveniently evade public responsibility for the substance or impact on society of the actions of their clients, or for actions they take on behalf of their clients. This is the essence of professionalism. Lawyers, with a monopoly of training and certain skills, become the official intermediaries of society; they "make things happen." Being beholden to neither client nor cause, but only to a form of activity, a process, lawyers lay a strong claim to legitimacy, self-regulation, and autonomy.

The empirical and ethical problems with such a model are obvious. First of all, as Auerbach recognizes, it is not an accurate description of the corporate Bar, or for that matter, of other sub-fields within the Bar which have long specialized in subjects and causes and identified with particular classes of clients. There are lawyers who represent only plaintiffs in tort litigation and lawyers who represent only the defendants in most such cases: insurance companies. Patent lawyers represent mostly those seeking patents. Criminal lawyers represent those accused of crime, and almost never their victims. The forebearers of the corporate Bar long ago mortgaged their souls—making their condemnation of current radical lawyers for taking up too closely with the cause and lifestyle of *their* clients sound just a bit hollow. Given this prevailing norm of specialization, the image of the lawyer as a neutral craftsman and not a policy maker is difficult to accept. Auerbach is persuasive in arguing that "process [cannot] be divorced from substance" (p. 304).

Conceding that lawyers, as individuals and as a class, must bear some social responsibility for the policies they help to create, however, does not completely destroy the counterargument for maintaining an ideal of lawyers as an independent force, caretakers of a special concern with process, with how things are done. The perverseness of legalism should not blind us to the many virtues of a rule of law which emphasizes procedure over substance. There simply must be a mechanism of adjustment which prevents "procedural justice" from becoming an end in itself.

Despite the title, the book does not deal with social change, except inferentially. Its assumption is that the selfishness of the elite Bar, and the consequent deterioration of its moral authority, have had negative consequences for social change. This is plausible, but a much too simplistic and casual allegation. It remains for another book to analyze more precisely the consequences to society of a legal profession which Auerbach has described so well.

JOEL B. GROSSMAN

University of Wisconsin

Southern Governors and Civil Rights: Racial Segregation as a Campaign Issue in the Second Reconstruction. By Earl Black. (Cambridge, Mass.: Harvard University Press, 1976. Pp. xiv + 408. $16.50.)

The subtitle describes the subject matter. *Southern Governors and Civil Rights* is a study of racial segregation as a campaign issue in gubernatorial races in the 11 former Confederate states during the years 1950–1973. This is an analysis of gubernatorial campaign rhetoric; it emphasizes attitudes rather than actions. Although candidate rhetoric and gubernatorial performance may differ, the type of appeal that a candidate makes reveals a great deal about the values and expectations of his constituents.

Outlanders may perceive southern politics as contests to see which candidate can yell "nigger" the loudest; Black demonstrates that reality is considerably more complex. A large percentage of candidates do talk about other than racial matters—the class nature of politics or their plans for educational expenditures. Moreover, there have been changes through time. The *Brown* decision unleashed a tremendous flood of segregationist rhetoric, but this flood was already ebbing at the time of the passage of the Voting Rights Act of 1965. From that date on, there has been a marked decline in segregationist appeals. Finally, there is usually a difference of emphasis between the rhetoric in the Deep South (Louisiana, Mississippi, Alabama, Georgia, and South Carolina) and that in the Peripheral South (the states of Texas, Arkansas, Tennessee, Virginia, North Carolina, and Florida—all with a lower black population than the states of the Deep South). Campaign comments in the Deep South appear more commonly in the harsh, unvarnished form.

In the years before the *Brown* decision, 1950–1953, there was actually less emphasis on segregation in campaigning. White supremacy was an unspoken agreement, and few questioned the faith. Since it was futile for a nonsegregationist to run, it was less necessary for candidates to convince the voters that they were the toughest segregationists. Under these conditions, some discussion of economic policy could take place. The author sets up four ideological categories: a key element of each is the candidate's position on substantially increased spending for public education. Other ideological classifications are those of strong, moderate, and nonsegregationist. From the 1954 *Brown* decision to 1965, segregationist fortunes soared; nevertheless, strong segregationists did not have a 100 percent victory rate. Candidates with other orientations did reasonably well, particularly in the Peripheral South.

After two chapters from an historical perspective—one on the Deep South through the 23-year period and one on the Peripheral South through the same years—the author adopts a topical approach. The chapter on first primaries refutes any simple rule that a strong segregationist invariably beats a moderate segregationist; place, time, and special circumstances have their impact on the situation. In the Peripheral South strong segregationists appeared less frequently and fared less well. After 1965 the number of strong segregationists declined markedly in both regions, and there were impressive victories by nonsegregationists. In a few instances a strong segregationist who was vulnerable to the charge of corruption lost to a candidate taking a more moderate stance, and, of course, incumbency was usually an asset.

In terms of the issues there was considerable overlap between racial and economic views. Strong segregationists were much more likely to favor the economic status quo. Nonsegregationists were frequently innovative in their economic policies and well disposed toward substantially increased educational expenditures. Since they foresaw a changing, nonrural South, they believed in the necessity for education to qualify citizens for the nonagricultural jobs expected to be available. In fact, the typical southern campaigner from 1966 through 1973 was one of the least traditional types—the nonsegregationist champion of increased educational expenditures. Some 45 percent of the region's candidates were of this combination, and it was the most common type in both areas of the South.

The author plots on a graph the racial stances of major candidates for governor in the first Democratic primaries. In the 1950–53 period almost all candidates were moderate segregationists. Their percentage declined steadily thereafter to around 15 percent in 1970–73. The fortunes of the strong segregationists approximate a bell-shaped curve–rising from nothingness in the first period, reaching a peak in 1954–1961, and a sliding to near oblivion in 1970–73. The nonsegregationists first become visible on the graph between 1958 and 1961 and move steadily upward to somewhat over 80 percent in 1970–73.

From 1953–1966, in the second or runoff primary the stauncher segregationists usually enjoyed the advantage. The explanation is simple. If worry over the maintenance of segregation was the chief preoccupation of the majority of white voters and they were given only two choices, they would naturally select the candidate taking a stronger stand on segregation. Less militant segregationists occasionally achieved victory when their opponent had a reputation for corruption or when they had the advantage of incumbency. Beginning with 1967 the fortunes of the strong segregationist declined even in the runoffs.

One might suspect that the general elections, now becoming much more common and important in the South, offered one last chance to kill off candidates who failed to take a sufficiently strong stand on segregation. But it did not work out this way. With the good-will value of the Democratic Party label, even in those instances where a Republican nominee made more ferocious segregationist noises, party loyalty prevailed.

Finally, Black engages in some reflections on the cause of the racial change. He attributes some small importance to changing demography: a much higher percentage of southerners now live in urban areas than in the predominantly black rural areas of a few years ago. He attributes greatest importance to federal intervention in the form of the Civil Rights Act of 1964, the Elementary and Secondary Education Act of 1965, and the Voting Rights Act of 1965. Such intervention relieves local leaders of the burden of implementing a deeply resented change. Finally, there is the re-enfranchisement of blacks. The ballot may not automatically allow blacks to achieve the goals they want, but the presence of a large black electorate makes it poor politics to wage an anti-black campaign. Nonsegregationists are not necessarily racial liberals; they may merely be people too prudent to insult, attack, and dismiss a large fraction of the electorate. Also, public opinion polls indicate a liberalization over time on racial matters by southern whites. Finally, Black suggests that the South now has a new generation of political leaders. They are not people who have been born again and dreamed a dream of a higher degree of political righteousness. Rather some of the older leaders came into prominence about the time of the Little Rock incident, assumed a posture that capitalized on the attitudes of that era, and have not been able to adapt to a changed situation. By contrast the younger politicians have faced no such problem and have easily adapted to the new climate of the times.

The first reaction to the book on the part of the reviewer–who lived and taught in the South during the entire period covered–was disappointment that the author was telling us nothing new. Black's sources are newspapers, and any southern political scientist given to reading would have intuitively reached about the same conclusions. However, mature reflection suggests that this is a superficial judgment. Only a tiny fraction of the book's potential audience will have the reviewer's life experience. An appalling number of members of our profession were not old enough to vote in the 1950s. Also, there is that majority of our colleagues to whom the vast area between Alexandria, Virginia, and El Paso is wholly unfamiliar territory except for a convention visit in New Orleans. To this majority of the profession *Southern Governors and Civil Rights* can be tremendously instructive.

In a more fundamental sense the volume is of value to all of us. The painstaking coverage of all the campaigns of the period demonstrates an admirable command of fact. Moreover, the author marshals his facts so as to test various theoretical propositions, including some advanced by Key in *Southern Politics*. The book will be a liberal education to those with a modest familiarity with the subject. Specialists will find it of value since it will challenge them to be sure to ask the right questions.

DONALD S. STRONG

University of Alabama

Edward Kennedy and the Camelot Legacy. By James MacGregor Burns. (New York: W. W. Norton, 1976. Pp. 383. $11.95.)

It is easier to say what this book is not than what it is. For example, it presents considerable information about Edward Kennedy's policy positions, staff, and activities in the Senate, but it is not a study of legislative leadership in the Huitt-Fenno tradition. This is evidenced by the fact that, while the book devotes six pages to how Kennedy won the majority whip's post in 1969 and lost it in 1971, it gives a scant paragraph to what he did with the job while he had it.

So it is a book about presidential politics. Yet, although published in a presidential year, it is not a campaign biography like Burns's 1960 book on John Kennedy–if for no other reason than the contradiction of writing a campaign biography about a noncandidate. Nor is it an occasion-less political biography in depth such as Burns's prize-winning volumes on Franklin Roosevelt; for in 1976 Edward Kennedy's life and career were presumably only at midpoint.

However one may classify the book, I surmise Burns chose to write it when he did for two reasons: first, because Kennedy just might have become a presidential candidate in 1976 and may yet become one in some future year; and, second, because Burns felt the time was right to restate his strong views on how presidents ought to behave and to add his judgment that Kennedy's capacities and inclinations place him at the forefront of those who would behave that way if they were elected.

Burns's convictions about the critical need for strong presidential leadership of the Congress, the bureaucracy, and the parties have been vigorously set forth in his earlier books, and this one adds little new to his case. What is new is his conclusion–slowly approached and cautiously phrased–that Kennedy could and would provide the kind of leadership the nation needs.

Burns bases this conclusion on several grounds: his approval of most of Kennedy's stated policy positions; his admiration of their boldness and innovativeness; his enthusiasm for the eloquence and vigor with which Kennedy had pursued them in the Senate; and his conviction that Kennedy's basic personal character is right for the job.

To most Kennedy-watchers, of course, this is the key issue. It is compounded by questions about Kennedy's sporadic slumps and erratic behavior, his wife's psychological problems, and, above all, Chappaquiddick. Burns deals with each of these questions mainly by citing at some length what various commentators have said about them. He himself offers no firm and clear judgment about any one of them, but his summary conclusion is that whatever mistakes Kennedy may have made did not stem from immutable basic character flaws, and that, tempered by the fires he has passed through, Kennedy is now ready to be the kind of president the nation needs.

The case Burns makes for Kennedy cannot be either proved or refuted in any wholly satisfactory way, and I shall attempt neither. In any event the issue may be irrelevant. Burns quotes Kennedy himself on why that may be so: "Some people say that 1972 is the year I must make a move for the Presidency, or 1976, or 1980. But how do I know that some young fellow . . . won't suddenly come on the scene and make everybody forget that anybody ever considered Ted Kennedy for the Presidency?" How indeed? Jimmy Carter may be that "young fellow," and if he is, a Kennedy presidency will not be possible until 1984 at the earliest. To be sure, Kennedy will then be only 52 years old, but how will eight more years' Senate service under a president of his own party affect his desire and prospects to become the kind of president Burns believes he would be?

That critical question cannot be answered for years to come. The fact that it must be raised now illustrates the hazards Burns encounters by hanging his general ideas about presidential leadership on a brief, however measured and restrained, for a potential candidate whose day will come, if ever, years from now.

In short, Burns on presidential leadership is always worth reading, and he remains a significant voice in the continuing discussion of the issues involved. But Burns on Edward Kennedy in 1976 is a *pièce d'occasion* for an occasion that has not yet and may never take place.

AUSTIN RANNEY

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Sectionalism, Politics, and American Diplomacy. By Edward W. Chester. (Metuchen, N.J.: Scarecrow Press, 1975. Pp. 362. $12.50.)

Chester has written a useful but unsatisfying study of the impact of sectional and political forces on the substance of American foreign policy. His book is useful because it provides a brief chronological overview of the major events of American diplomatic history and a synthesis of the leading views on the relevance of regional and partisan forces for understanding those events. The integrative effort is reflected in an extensive bibliography valuable for readers wishing to dip into the waters of diplomacy.

The book is unsatisfying for a number of reasons. On the one hand, it is the sort of work that behaviorally inclined faculty of the 1950s used to illustrate the frailties of conventional scholarship. It lacks clear definition of the essential concepts; Chester cannot even decide what are the basic regions. Starting with the nine regions used by the *Statistical Abstract of the United States,* he notes that "simply by transferring a single state from one group to another here and there, it is literally possible to produce dozens of arrangements, one of which is probably just as logical as any other" (p. xi). He then proceeds, for example, to use the "Old Northwest," the "Middle West," the "Great Plains," the "Upper Mississippi Valley," the "Great Lakes States," the "North Central States," the "East Central States," and the "West Central States"—all of which seem close to what is often called the Midwest—to catalogue regional variations in popular sentiments about numerous historical events. Perhaps these usages make stylistic sense or are simply taken over from the authors in whose work the treatment rests; the effect, of course, is to confuse the reader and to frustrate Chester's hope "that one may detect significant sectional differences of opinion . . . while yet employing fluid categories" (p. xi).

The assessment of opinion poses additional problems. Lacking poll data on individual attitudes, it is not entirely unreasonable to make use of roll call votes in Congress and of newspaper editorials as guides to regional viewpoints, but simply, without elaboration, to equate these expressions with "public opinion" is to invite all the distortions that survey research has so clearly revealed. Incidentally, the work of such historians as Lee Benson, Jerome Clubb, Samuel Hays, and others, who seek to avoid precisely these sorts of erroneous inferences, is notably absent from the book. Finally, in spite of such invitations to caution, Chester tends to advance sweeping but unsubstantiated generalizations about particular events: "the opposition of Van Buren and Clay to annexation [of Texas] was instrumental in denying both the presidency" (p. 59); because "only three northwestern Senators" voted against repeal of the Elgin-Marcy Treaty, it is clear that "American farmers were obviously resentful about Canadian peas and other crops" (p. 90); "The hostile reaction [to the publication of the Zimmerman telegram] was particularly strong in the Southwest, previously rather indifferent to the war and only mildly anti-German; the Southeast and Gulf coast were also incensed by the revelations of this fantastic plot" (p. 175); "[North Dakota Republican Senator Gerald] Nye, however, was unquestionably instrumental in strengthening isolationist sentiment in this country and laying the groundwork for the neutrality legislation of 1935–1939" (p. 206). While these propositions may or may not be true, no evidence or citation is presented to sustain them for many other similar assertions.

On the other hand, though he is prepared to generalize about specific events, Chester is unwilling or unable to do so on the big questions that are his central concerns. Neither the individual chapters on the several historical epochs nor the conclusion advances any firm propositions about the place of sectionalism or politics in foreign policy making. Indeed, the final chapter begins with an admission of failure: ". . . once one passes beyond the political breakdown of votes in Congress. . . , it is clear that other factors in addition to political considerations have influenced sectional attitudes toward diplomacy. Perhaps the most important of those have been economics, race, ethnicity, and metropolitanism vs. ruralism. . ." (p. 274). Such a catalogue will scarcely startle political scientists (nor, probably, Chester's fellow historians). In sum, the author, like others before him, has identified some of the crucial factors, but he has made no real progress in untangling their relative importance as influences on specific events, much less on the broad sweep of American foreign relations.

LEROY RIESELBACH

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The Making of Congressmen: Seven Campaigns of 1974. By Alan L. Clem. (North Scituate, Mass.: Duxbury Press, 1976. Pp. ix + 275. $5.95, paper.)

Perhaps the biggest hurdle for the study of political campaigning is that its subject matter is so fascinating. As a result, researchers can easily remain intrigued by describing the interplay of personalities, the development of the issues, the unexpected happenings, while the need for subjecting this fullness of detail to hypothesis-testing seems dull by comparison. To an extent, this is true of *The Making of Congressmen.*

The book is a collection of seven case studies of 1974 congressional campaigns, each written by a different set of authors, bracketed by Alan L. Clem's chapters designed to introduce the major concepts and sum up the results. The campaigns range widely in geographical setting and characteristics, and the quality of the analysis and writing ranges widely as well.

Two excellent chapters are contributed by Robert Lineberry and associates and William Cavala. In the former, describing a newly competitive district in Texas, the authors provide a very insightful (and delightfully well-written) discussion of the effects of district characteristics—ethnic composition, size, population distribution, economic base—on the campaigners' decisions and successes. The district they studied lent itself well to their purpose because one of the candidates was a political newcomer; thus the reasons for the candidate's strategic choices and contacts, reasons that are usually obscured in the well-developed patterns of a veteran campaign, became more visible.

Cavala's perceptive analysis of a California campaign focuses on problems that occur when a campaign ages, when a long-term incumbent suddenly becomes vulnerable to challenge because of national political forces and changes in the district. Cavala's main concern is the effects of incumbency on both the campaign and the election outcome. It is amazing that despite the obvious importance of the matter (upwards of 90 percent of congressional incumbents are re-elected each year), so few political scientists have examined the strengths and weaknesses of incumbent status in campaigns, and whether it is the status itself or the personal effectiveness of those who become incumbents that adds to their staying power. Cavala tests this question by looking at a race where the resources usually associated with incumbency—name recognition, money, organization—were also possessed in nearly equal measure by the challenger. Therefore he is able to identify other key differences between incumbents and challengers, and to offer useful conclusions about the factors that cause other politicians to judge a race competitive or secure.

The qualities that make these two chapters good—their concern with more general concepts and hypotheses about the campaign process—are not found as frequently in the remaining five case studies. Several are quite interesting: it would be difficult even for Nero Wolfe to bypass anything entitled "The Case of the Right-Wing Urologist" (by Charles Bullock and Catherine Rudder). However, these remaining chapters are predominantly descriptive, concentrating on the uniqueness of each race rather than on its relationship to the nature of campaigning in general. Because of the wealth of detail the authors offer, students of campaigning can draw material from each of the cases that may support or fail to support a variety of hypotheses, but the authors themselves often neglect to make these linkages.

The introductory and concluding chapters by Alan L. Clem are ambitious in their goals, but read more like an overlay than a genuine effort to draw these cases together and to demonstrate their contribution to the study of campaigning. Perhaps this is because Clem tries to do too much. He suggests that the book will not only record and capture the flavor of the seven campaigns, but will also provide comparative campaign analysis. He offers the use of six "theories" that are not really elaborated upon, and fully 14 variables that he believes are determinants of campaigning. Perhaps because of the enormous size of such a framework, the case studies do not use very much of it systematically. The concluding chapter alternates between very specific details of the various cases and very general conclusions that are not always based on the material the studies present. For example, Clem asserts that the book is intended "to assess the relevance of campaigns to the representativeness of Congress" (p. 235), but the case studies are not really designed to address this question, and neither they nor the conclusion provides any real answers.

Case studies such as these can be quite helpful for several reasons: to illustrate the wealth of diversity in campaigns and their centrality to the democratic process; to provide ideas for further investigation (such as, in these chapters, the uses and hazards of incumbency in campaigning); to hold the attention of an undergraduate audience and help the instructor convey more general concepts about the process of campaigning. The studies in Clem's book serve these purposes very well.

But to explain why campaigning works as it does, researchers must advance beyond the case study method. We need a commitment to comparative research, not only within national boundaries but cross-nationally as well. Now that case studies such as these have provided ample material for generating hypotheses, we need more systematic hypothesis-testing. Smaller-scale comparative studies can offer insights into the campaign organization itself, but only through large-scale research can the effects of different types of party systems and electoral rules be tested. Several of the important variables need to be clarified and made operational. For instance, several chapters argue that the "political culture" of the district has affected the decisions that campaigners make, but as long as that term remains diffuse and undefined, covering everything from a district's political history to current public opinion, the contention cannot be falsified. Finally and most important, campaigning research can make much greater use of several theoretical approaches, recognizing that campaign behavior is a subset of political behavior. Literature on organization theory and decision making has already proved helpful, as should small group research and learning theory.

The contributors to this book should be commended for attempting to reach beyond the limitations of the case study method, and for producing some very interesting analyses. But perhaps because many campaigns are so enjoyable to read about, campaign researchers have a lot left to do.

MARJORIE RANDON HERSHEY

Indiana University

Blacks in Power: A Comparative Study of Black and White Elected Officials. By Leonard A. Cole. (Princeton, N.J.: Princeton University Press, 1976. Pp. xiv + 267. $13.50.)

Extensive change occurs in so many sectors of American social and political life that there is a tendency to be rather blasé about it. And yet the changes are often quite extraordinary and deserving of careful assessment. Leonard Cole has written a book about a significant element of political change in the United States which follows from a series of broader social transformations. He has examined the movement of black Americans into the electoral arena, specifically as officeholders. He attends principally to the experience in the state of New Jersey, but much of his analysis clearly has implications for the national experience.

The methodology underlying this study comprises these elements. The author located the 16 New Jersey cities with populations of 25,000 or more and with significant numbers of black residents—here taken as 15 percent of the total or more. These 16 cities were governed in 1972 by 135 elected executive and legislative officials, 31 blacks, 104 whites. These officials were interviewed in a semistructured format. Questions dealt heavily with perceptions of public policies affecting race. A parallel survey of a cross section of the general public in the 16 cities was conducted by telephone. The author has, then, data on both white and black elected officials and their constituents, data which include perceptions of public policy in the area bounded by racial concerns and which, for the general public, include evaluations of the performance of elected black officials.

On the basis of the interviews and observations of public policy development in the 16 communities, the author makes the following concluding observations: (1) The presence of significant numbers of black elected officials in these cities contributes importantly to getting concerns of the black community on the formal agenda for legislative or executive action. Once on the agenda, the various proposals often receive biracial support. (2) Black elected officials influence significantly the number of blacks appointed to governmental positions and the levels of these appointments. (3) Apart from specific policy actions, black elected officials serve to sensitize white officials with whom they sit as to the problems and concerns of the black community. (4) The presence of significant numbers of elected black officials is symbolically extremely important to the black community and encourages communication flow which would otherwise be hard if not impossible to attain. (5) The elected black officials serve as role models for the black community. (6) The presence of significant numbers of black officials in these 16 New Jersey cities has served to create a sense of normalcy about blacks holding elected office. Subsequently, racial identity has become less consequential in shaping patterns of popular support.

Cole provides a valuable discussion of the experience of blacks holding elected office at the community level in the urban north. However, the study does manifest one notable weakness. The questionnaire employed in the leadership interviews does not permit any serious assessment of ideological differences among black leaders or any assessment of contrasting styles of race leadership. It may well be that the typologies developed earlier by James Q. Wilson, M. Elaine Burgess, Everett Ladd, and others are essentially inapplicable to this contemporary (1970s) urban and suburban New Jersey environment, but the author is not able to offer any alternative typology. The chapter on "philosophy and ideology" and much of the related commentary, as a result, lacks a handle. It manifests a kind of analytic blandness and imprecision. And this particular omission in an otherwise commendable study forecloses certain potentially valuable points of analysis—such as the question of the relationship of ideology and style of interacting sets of black and white leaders to policy formulation and policy execution.

EVERETT C. LADD, JR.

University of Connecticut

The Role of the Supreme Court in American Government. By Archibald Cox. (New York: Oxford University Press, 1976. Pp. vii + 118. $6.95, cloth; $1.95, paper.)

With this slim volume Archibald Cox continues to pursue the theme begun in his earlier *The Warren Court*, a concern for the long-range institutional consequences of the use and abuse of constitutional adjudication as an instrument of social reform. Originally delivered as the 1975 Chichele Lectures at All Souls College, Oxford, the book addresses the question: How much of America's affairs should be handled as matters of constitutional law? Intended for a general university audience, Cox's analysis proceeds on three levels. The first is expository, surveying the case law in several areas. The second is jurisprudential, exploring the problems of when, how, and why the Court should substitute its judgment for that of elected authorities on difficult and debatable questions of policy. The third is theoretical, inquiring into the impact upon the Court's legitimacy of judicial efforts at social engineering.

Cox introduces the problem of the Court's role in American government in a context with which he had some experience, the Watergate tapes case. He then examines the origin of the Court's authority to resolve such political conflicts. After a journeyman exegesis of *Marbury*, he ultimately concludes that, in a system of government as extraordinarily complex as the American, there must be some umpire to arbitrate with finality the myriad conflicts which will arise. Such an argument from necessity has a certain pragmatic force, but in advancing it Cox never directly confronts the fundamental issue: Granted that some umpire is necessary, why should it be a small band of unelected, life-tenured, irresponsible judges?

Having lodged constitutional interpretation in the Court by default, Cox turns to the question of judicial method. Which of the dissenters in *Lochner*, he asks, was right? Harlan or Holmes? Did the Court err in its empirical assessment of the need for maximum-hours legislation or in undertaking that assessment? This tension between the precepts of popular sovereignty and the philosophy of natural rights is fundamental and on-going within the American political system. To illustrate, Cox discusses the development of freedom of expression, right to privacy, and equal protection case law.

Cox has reservations about the Court's handling of each of these areas. First Amendment interpretation has encouraged the publication of the vulgar, has lowered the tone of public discourse, and been at odds with reason and civility. The right-to-privacy decision in the abortion cases is so far from enunciating a judgment based upon abstract principle as to read like a set of hospital regulations and, as such, expresses nothing more than personal preference. The vigorous use of the Constitution to promote equality—handling lawsuits in the mode of social legislation by attempting to strike accommodations between competing group interests—has been marked by inconsistency, ambiguity, and partiality.

While one may differ with specific interpretations, Cox reminds us that the price for the modern American expansion of civil rights and liberties has been the commission of more and more questions of public policy to the judiciary and the denigration of the democratic processes. This dilemma has been exacerbated by the transformation of constitutional interpretation from a prophylactic device to a vehicle for mandating particularized, affirmative actions. This invitation to the disaffected to secure by adjudication what they cannot obtain politically threatens government by judicial fiat for, as Cox's own examples make clear, such power cannot be logically confined to the Supreme Court but must extend to the lower federal judiciary and the state courts as well. Thus, today we find courts—often single judges—across the country establishing educational personnel, fiscal, and tax policies regulating the lives of millions of people without a voice in those decisions. And at what cost to constitutionalism?

Inquiring into the sources of judicial legitimacy, Cox notes the courts cannot go it alone. The Supreme Court especially is uniquely dependent upon popular consent if it is to command acceptance. Cox is not criticizing particular results, but he wonders if a generation of judicial activism supported by inarticulated premises, questionable assumptions, faulty logic, idiosyncratic history, and capricious reading of precedent has not seriously impaired the capacity of the Court to perform its traditional, more modest, but no less essential functions. Candidly, he admits that there are no ready measures available to answer that question. But he concludes that the modern Court's tendency to behave as a mere clientele agency has gone too far.

What clearly bothers Cox is the tendency of excessive politicization to feed upon itself. Nixon's "value packing" of the Court, he suggests, was a direct consequence of the Warren Court's disregard for the traditional confines of the judicial process, its movement away from negation to initiation. Now, although the direction of judicial decision has been slowed if not reversed, there is little indication that the new justices entertain a limited conception of their role. But the creation and continued exercise of an expansive judicial function presents problems of competence and principle of such magnitude that they may overwhelm the institution of the Court itself.

This is a powerful plea for judicial self-restraint advanced by one who has enjoyed a rare opportunity to develop extraordinary insight into these problems. Professor Cox has been one of the true martyrs of the American constitutional faith, and for that reason one treats him negatively with reluctance. Nevertheless, this book suffers from a variety of shortcomings. Its complaint is neither novel nor is it ever clear whether its prescription for judicial craftsmanship proceeds from a philosophic or a merely prudential concern. Is the Court to restrain itself because that is good democratic theory or because discretion is the better part of valor? The fundamental assumption, that there is a relationship between judicial craftsmanship and popular perceptions of the Court and, thus, between adherence to traditional forms and the Court's legitimacy, is never examined, let alone proven. The volume is so replete with minor errors (e.g., incorrect dates, p. 16; inaccurate citation, pp. 28, 29, 98; attribution of Burger Court decisions to the Warren Court, pp. 49–50; consistent misspelling of the Chief Justice's name) as to undermine confidence in Cox's scholarship. In short, students of American constitutional law will find this presentation enlightening but, ultimately, wanting. Most disappointed will be those who sympathize with Cox's indictment, but wish that the case had been better presented.

RICHARD FUNSTON

University of California, Davis

The Tides of Power: Conversations on the American Constitution. By Bob Eckhardt and Charles L. Black, Jr. (New Haven, Conn.: Yale University Press, 1976. Pp. ix + 225. $10.95.)

The friendship and political conversations between Representative Bob Eckhardt, a Texas Democrat, and Professor Charles Black of Yale have stretched over nearly a half century, from their childhood days in Austin, Texas. Although a charming idea, the publication of several of their conversations (edited from tape recordings) about the American constitutional system bears the ragged edges that one would expect of such commentary. The focus would have been sharper, the analysis more orderly, the issues plumbed more deeply, and the text reduced by half if these two old friends had co-authored an essay on the workings of the separation of powers. As in any conversation, the level of discourse varies widely—from an elementary discussion of the congressional committee system to an erudite exchange about the appropriate burden of proof in impeachment proceedings.

The most provocative conversations clearly occur in the first four chapters on the "constitutive force of the American Constitution," the weaknesses of Congress, the strengths of the presidency, and impeachment, impoundment, and executive privilege. The three remaining chapters seldom engage the reader, covering familiar ground about the congressional committee system, the electoral college, and the need for Congress to obtain better information and ideas in addressing public issues. (Neither of the conversationalists acknowledges that this latter question has recently been faced in the debate about the proper mission of the Congressional Budget Office, which in operation has been increasingly pressed to meet the immediate information needs of Congress at the expense of its long-term "think tank" activities.)

A chapter on the judiciary witnesses Black, the sometime activist constitutional lawyer, condemning the Warren Court's tendency to reduce the standing requirements in constitutional cases. Black acknowledges that in such areas of Warren Court activism as free speech, race relations, and legislative apportionment, the judiciary is well equipped to act. But for the next decade, "the dominant strategic question about justice" will be "the poverty problem," and there "the courts have little to contribute" (p. 124). Eckhardt, the congressman, is less suspicious of judicial review, welcoming the Supreme Court's liberal decisions, arguing that "there is something salubrious about permitting generous rules of standing where the issue is one of common concern to a great body of the public" (p. 112), urging that judicial modesty is necessary only when judges lack the intellectual tools or legal remedies needed to act, suggesting that even in the area of poverty the justices might act as a goad, and pointing out that they should have done so in the school financing case (*San Antonio Independent School District v. Rodriquez,* 411 U.S. 1) by simply allowing students from poor districts to enroll at their own discretion in any other district where they believed they would obtain a better education.

The main concern of these conversations is, however, "*constitutive* constitutional law rather than *limiting* constitutional law"—the American "system for creating offices, for choosing the occupants of those offices, for empowering the occupants of those offices. . ." (p. 9, authors' emphases). It is not only the text of the Constitution, but public opinion, current practice, and fixed customs or habit ("the done thing") that define the constitutive Constitution.

Black and Eckhardt agree on the outstanding feature of our constitutive Constitution. Black: "The text of the Constitution places all the power in Congress, but the structure of the presidency is such, and the structure of Congress is such, that the power tends to flow from Congress to the presidency, and that leadership tends to be looked for in the President" (p. 36). Eckhardt: "The greater power resides in Congress, but the greater exercise of it has come to be engaged in by the President" (p. 26).

Both Black and Eckhardt believe that there is no greater textual warrant for presidential power in foreign affairs than in domestic matters. Both favor more extensive use of treaties, to insure greater congressional participation in foreign relations and to give legitimacy to American commitments. Both agree also that the president has no constitutional war-making power, and that the War Powers Act should therefore be read as a congressional delegation to the president of a limited authority to make war. Eckhardt additionally dissents from the conventional view that the need for secrecy and swift initiatives necessarily empower the president in foreign relations; this argument erroneously assumes that democratic nations can or must conduct foreign policy in the same manner as totaliratian states.

Congress could recover some of its power, Black suggests, by developing a convention—similar to senatorial courtesy—generally to override those presidential vetoes which are not episodic events, but connected actions preventing broad policy initiatives by Congress and eventually forcing presidential preferences. Eckhardt deems the idea impractical, for in voting on vetoes party affiliations, ideological preferences, and constituency outlooks will triumph over any concern to strengthen the legislative institution. Eckhardt several times suggests that a stronger majority party caucus would help redress the imbalance between the two branches, but the preoccupation of members of Congress with home district electoral strategy and the fear, expressed by Black, of a return to "czarist" Speakers of the House seem insurmountable barriers.

In the end, neither Eckhardt nor Black finds much prospect that Congress will overcome its self-imposed institutional decentralization or its narrow constituency orientation. Black: "Eckhardt, you seem to me to be, in one case after another, preaching counsels of despair and prophecies of doom. What you are saying in one context after another is that Congress is just disabled from doing anything very important" (p. 53).

Indeed, the whole drift of these conversations is captured by Black's characterization of Eckhardt's position about foreign policy: "You are saying . . . that the President can and will respond to the long-range interests of the country better than Congress will. That is to me a sad conclusion . . . . It would constitute something very near a totally satisfying rationale for a continued growth in the power of the President. . ." (p. 53). Nothing said by either Black or Eckhardt leads the reader to any other conclusion.

DAVID ADAMANY

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Women in the Professions. Edited by Laurily Keir Epstein. (Lexington, Mass.: Lexington Books, D.C. Heath and Company, 1975. Pp. 136. $15.00.)

Women in the Professions is a collection of articles chosen from papers presented at a two-day conference on women in higher education and the professions. The papers vary both in quality and potential reader interest; the criteria used for inclusion is difficult to ascertain, especially since the editor does not make a serious attempt to identify a unifying theme and it thus becomes necessary to discuss the articles individually. Two of the more stimulating or intriguing articles merit special attention.

Cynthia Fuchs Epstein in "Success Motivation and Social Structure: Comments on Women and Achievement" challenges such writers as Richard Lester and Marina Horner who, Epstein states, have arrived at conclusions that divert attention from those responsible for discrimination and prejudice. The Horner and Lester studies imply that it is useless to try to do much about discrimination, since women don't want to succeed and have not yet received the proper socialization to compete with males. Eptsein correctly points out that women with positive reinforcements who experience the kinds of support routinely offered to males *can* and *do* succeed. I have argued elsewhere that what is mistakenly identified as sex differences may instead be the result of opportunity structures and hierarchical behavior patterns. Feminists have too readily accepted role determination and socialization as full explanations for the status of women. They popularize programs such as assertiveness training without also emphasizing the realities of discrimination and the limitations of any one individual attempting to challenge the larger organization. The mouse taught to roar may become the mouse drawing attention to herself; without structural support she may simply be stomped.

Carolyn Shaw Bell has written a provocative paper entitled "Alternatives for Social Change: The Future Status of Women." Bell correctly points out that 32 million working women won't go home again and that the current employment challenge is basically one of ending the sex-typing of jobs. Bell states that occupational segregation is not a function of discrimination but of the division of labor at home. This division means that women employees will have less experience because they leave their jobs when they have children or when their husbands move. Thus women are often relegated to jobs that do not require a large investment on the part of the employer. Bell makes suggestions to remedy the situation, such as marriage contracts which explicitly identify the division of home responsibilities. She also advocates that working couples contribute equally to childcare insurance so that in cases of divorce childcare could be hired, or one parent would be paid to stay out of the labor force to care for the child. The mind boggles at what the premiums would be with this additional stimulus to our divorce rates. One of Bell's more original suggestions is levying a flat "head tax" on men to pay welfare benefits to families headed by women. This would erase income differentials between poor children with only a mother and well-off children with two parents. The only exceptions would be sterile men and men engaged full time in child-care and home responsibilities. It can be disputed whether or not this idea is more questionable because of its sex discrimination or because of the invasion of privacy involved in proving infertility to the IRS.

Bell would also use other taxing policies to redirect productive activity. Employers could be taxed when they employ more men than women. As another possibility, men could simply be taxed more than women. Both steps would encourage men to consider employment at home as an alternative occupation. Although the usefulness of taxation as an instrument of restructuring society is well established, the present political realities make these alternatives highly unlikely. A practical problem is that if the amount of the tax is small (such as the difference in car license fees between small and large cars) it won't have the necessary impact to achieve its intended purpose. If the tax is big, in the short run it could have disruptive macroeconomic effects which could easily penalize the very persons it is seeking to help. The sudden removal of male workers whose experience gives them high productivity could easily lower total real income. It is unlikely that women could gain a larger actual slice from such a shrinking pie.

Although it is impossible to review all the articles here, two others deserve mention: John J. Stucker's excellent chapter on women as voters and Jewel Prestage's seminal article on black women office holders.

NAOMI B. LYNN

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Presidential Spending Power. By Louis Fisher. (Princeton: Princeton University Press, 1975. Pp. xiii + 344. $12.50.)

James Madison provided in the *Federalist* papers (Number 58) an intriguing projection of the possibilities of congressional financial control over the executive. The power of the purse, Madison wrote, "may, in fact, be regarded as the most complete and effectual weapon with which any constitution can arm the immediate representatives of the people, for obtaining a redress of every grievance, and for carrying into effect every just and salutary measure." In this landmark study of the executive-legislative tandem and Congress's stewardship of the spending power, Louis Fisher concludes that the Madisonian vision of legislative assertion has been overcome by the actuality of a gross expansion of executive power to the point that "instead of public laws determining how taxpayer funds are spent today, the crucial commitments are often made by administrative officials" (p. 257).

Among the forces that contributed to this transformation was the establishment of an executive budget that Speaker "Uncle Joe" Cannon warned would signify the surrender of the most important element of representative government. The principal administrator of that budget, the Bureau of the Budget (now the Office of Management and Budget), since its modest beginnings, has undergone magnitudinous accretions of power. For example, it has grown from an early staff of about 40 to 600; its responsibilities for promoting economic stability have increased; it oversees grants-in-aid to the states and localities; it is increasingly involved in federal personnel and procurement policies.

This book deftly traces Nixon's transformation of the Bureau into the OMB, a move foreshadowed by both the Brownlow Committee and the second Hoover Commission, which urged an expansion of the Bureau's management functions. Nixon's revamped OMB quickly became a major weapon in his confrontations with the Democratic Congress, a development readily foretold when the new emphasis of the agency was declared to be "assessing the extent to which programs are actually achieving their intended results, and delivering the intended services to the intended recipients."

The lump-sum appropriation, preferred by the Brownlow and Hoover studies (as well as by the prestigious director of the Budget Bureau, Harold Smith, and many others) to what were perceived as excessively limiting line-item appropriations, also contributed importantly to the aggrandizement of presidential spending power. Likewise, other techniques adopted to respond to the conflicting needs of administrative flexibility and congressional control have tilted in the president's favor–reprogramming, for instance, which gives executive officials latitude in shifting funds within an appropriation account, from one program to another. Reprogramming is especially important in military appropriations, and, despite procedural restrictions, has become a means for executive officials to avoid the customary authorization and appropriation stages. Thanks to reprogramming, officials need obtain approval only from certain subcommittees with which, in all likelihood, their relations are aglow with mutuality. Transfer authority, enabling agency officials to move funds from one appropriation account to another, is also depicted as contributing mightily to the expansion of executive power.

The most controversial instrument of financial management in recent times, impoundment, is astutely treated by Fisher. Impoundments come in many shapes and sizes, and, although used repeatedly, especially in modern presidencies, have evoked low-boiling altercations between the branches. Although Nixon's OMB Director Caspar Weinberger contended that his administration's impoundments were similar to Jefferson's, Nixon in fact exceeded all his predecessors in moving to terminate programs and in challenging the right of Congress to make policies and decide priorities.

Congress resorts to other devices of financial management that are tantamount to empowering the president to apply impoundments. For example, Congress imposes spending ceilings, but leaves to the president the selection of programs to be trimmed. Richard Nixon was among those presidents repeatedly favored by this practice, thanks to Congress's disinclination to shoulder the political blame for cutting programs. Setting the public debt limit appears to be an act of congressional assertion, but a presidential administration, in the course of observing it, may give preference to its own programs and priorities at the expense of those enacted by Congress. Frequently, Congress, in setting program standards, empowers the president to shut off funds because of violations. Thus the 1964 Civil Rights Act empowers the president to withhold funds from federally financed programs in instances of racial discrimination.

The major growth sectors of fiscal management include covert financing and confidential funding, which prompt Fisher to make an intriguing analysis of the intricacies of the CIA's use of its extraordinary financial freedoms, including its secret funding of private organizations, its military operations in Laos, and domestic intelligence gathering. This book analyzes the inadequacies of congressional oversight of these out-sized gifts of power.

In responding to the task of prescription, Fisher remains practical and astute instead of coming down on the side of a rebirth of Madisonianism as the antidote to the governmental system's troubles. He rejects the alternative of hobbling the executive by binding the president in reams of confining regulation. He recognizes the need for administrative discretion and flexibility. A key to a better future, as Fisher perceives it, is the career executive official, endowed with integrity, and, consequently, a fit repository of legislative trust. Among other things, Fisher would seek to develop better techniques of accountability, applicable not only to the executive, but also to Congress which has the troublesome habit of hiding its accountability in the far recesses of its subcommittees.

This surely must be the most informed and perceptive book ever written on federal financial administration. It will long reign as the standard work, for it rests on fine scholarship and is both historical and contemporaneous in scope.

LOUIS W. KOENIG

New York University

The Union Politic: The CIO Political Action Committee. By James C. Foster. (Columbia: University of Missouri Press, 1975. Pp. 247. $12.00.)

James C. Foster has written a competent, useful narrative of PAC, the CIO's Political Action Committee, from World War II to the 1955 AFL-CIO merger. Lacking theoretical pretensions, *The Union Politic*'s important contribution lies in the insights about union electoral activity that Foster's data suggests.

Foster's persuasive central thesis is that both PAC's effectiveness in election campaigns and its political radicalism were substantially exaggerated, by contemporary friends and foes alike. Such illusions, however, are much less widely accepted now, and Foster makes no effort to develop a systematic explanation of union political behavior in American politics. In turn, this lack of theoretical focus mars the historial analysis itself. He discusses issues (e.g., the fear of a depression after World War II) and alliances (e.g., with the A.D.A.), evidently where they seemed most intuitively important, but then drops them, leaving their outcome unclear. Had Foster developed some broader propositions, e.g., about the causes and effects of PAC's alliance with the Democrats, these important narrative threads would not have been left dangling.

Despite these shortcomings, *The Union Politic* reports many facts of theoretical interest. In the 1940s, for example, PAC actively sought to extend the realignment of the national parties that occurred in the 1930s. For example, the CIO worked to eliminate the Wisconsin Progressives (thus strengthening liberal Democrats) and supported the fusion movement under Hubert Humphrey that produced Minnesota's Democratic Farm-Labor Party. Nor does Foster neglect the CIO's intimate alliance with the Democrats. In 1948, a union's support for President Truman had more influence on preserving its membership in the CIO than did any issue more directly related to collective bargaining or industrial relations. And, he argues, the CIO actively helped draft Adlai Stevenson as the Democratic nominee in 1952.

The Union Politic is at least as illuminating in detailing the process of routinization and rationalization by which the magnetic leadership of Sidney Hillman gave way to the more bureaucratic, professional organization of the early 1950s. And despite the vexing problems of demonstrating PAC's effectiveness in electing its candidates, Foster does offer considerable evidence from county, city and ward voting data which suggest that this process of rationalization did increase PAC's political impact. In my view, the most important point here is that these improvements in organization, strategy and tactics must be explained not by some inexorable behavioral uniformity, but as the result of deliberate reflection and self-criticism. In each case—be it greater attention to union wives, increased sophistication about racial tensions among union members, the new use of survey data, or the professionalization of PAC's financial, research, publication, and campaign activities—PAC systematically tried to learn from its mistakes.

Foster is at his best, then, in using archival data to illuminate the *intentions* of union officials as well as their observable behavior. More strictly behavioral research, of course, can show correlations between changes in union tactics and (controlling for other factors) electoral outcomes. But as the contemporary philosophical revolt against positivism has shown, we can scientifically theorize about such changes most effectively, we can account for future changes most economically and comprehensively, only if we take such intentions into account by viewing such *behaviors* as intelligibly motivated *actions*. It is the greatest merit of Foster's book that his data help us carry this task of theory building forward.

J. DAVID GREENSTONE

University of Chicago

Affirmative Discrimination: Ethnic Inequality and Public Policy. By Nathan Glazer. (New York: Basic Books, 1975. Pp. vii + 248. $10.95.)

Nathan Glazer presents a comprehensive critique of affirmative action in employment, education, and housing. The style is easy, the argument coherent. Unfortunately this work is a polemic which mixes closely reasoned and sound analysis with overstatements, misstatements and speculation to make the point that the United States is presently embarked on policy that is fundamentally at odds with the essentially sound and just consensus on ethnic policy that has developed over the past 200 years. That consensus, as Glazer ably develops it in his introductory chapter, is "that all [regardless of race, religion, or national origin] may be included in the nation, that they may not establish new nations here, and that they may, nevertheless, freely maintain whatever aspects of national existence they are inclined to" (p. 28).

Affirmative action in employment, conceived as a program to insure equality of opportunity and to seek out and prepare members of disadvantaged minority groups for better jobs and educational opportunities, served its purpose well resulting in a dramatic improvement of the economic status of those groups in the 1960s and early 70s. This was done, says Glazer, without violating the consensus. But affirmative action has been perverted into a program which insists on quotas in employment. Glazer marshals the statistics and examples to make this point, but the continuation of an unemployment rate among minority groups that is twice that among the white population throws doubt on the picture that he paints. And, while there is some evidence that in some areas federal agencies have enforced employment quotas, there remains considerable doubt that they are in fact indicative of governmental policy. Glazer cites the universities as an example of the application and implementation of such an alleged policy, and while he cites some horrible examples, they are the same examples that by now have become legendary in the literature on the subject and by no means suffice to establish the fact that the responsible agencies have in fact adopted a quota policy.

Glazer argues that school desegregation has been turned into a policy to achieve racial balance in the public schools. He bases his argument on his analysis of court decisions in which he repeats much of the misunderstanding, overstatement, and misstatements characteristic of antibusing critics. He states, for example, that "there is considerable room for doubt as to whether the Constitution actually mandates a system whereby every school shall have a black minority and no school shall have a black majority" (p. 119). Of course there is considerable doubt! Even a cursory reading of *Swann* v. *Mecklenburg* makes that clear. Glazer also states that the courts have abolished the distinction between *de jure* and *de facto* segregation, basing his assertion on an analysis of *Keyes* v. *School District No. 1. Denver.* But, as anyone who has read the decision knows, the Court, over the objection of Justice Powell, carefully maintained the distinction in that case and has maintained it since. Glazer next turns to the argument that the policy of racial balance is educationally undesirable. He points out quite properly that as yet there is no incontrovertible evidence that racial balance in the schools has a beneficial effect on the academic achievement of minority group children. He also cites testimony by students from the Berkeley schools before a Senate Subcommittee to the effect that integration had no beneficial effects on racial attitudes of the students. He might of course have cited the contrary testimony of Pontiac, Michigan students before a House Subcommittee. In fact, systematic studies of the effect of integration with or without busing, on racial attitudes are just as equivocal as studies of effects on academic achievement. The problem with all of these evaluative empirical studies is of course that they are inconclusive because the experiment has not yet been run long enough or under sufficiently controlled circumstances, and because of the epistemological problems inherent in all evaluative empirical research. But Glazer, instead of examining these problems, uses "research" and anecdotal "evidence" to support his conclusions—that busing is educationally undesirable.

Glazer finds a tendency towards adoption of similar policies in housing. The trend is toward a federal housing policy which will encourage (if not require) broader distribution of minority groups throughout the larger urban areas of the country. A further chapter is devoted to an analysis and explanation of "white backlash." Glazer argues that the reaction of the "later white ethnic groups" is motivated by their sense of the unfairness of the policies he has described and not by racial prejudice. There is much that is of interest in this chapter, but the judgment rests on the analysis in the previous chapters.

In the concluding chapter, Glazer asks how policies "which so sharply reverse the consensus painfully developed over 200 years of American history established themselves so powerfully in a scant ten years? . . . How do measures which most people oppose become imposed upon them in a democracy?" The answer he suggests is that it has been done by the courts, executive and legislature impelled by a misguided "educated opinion which is convinced that morality and progress lie on the side of the broadest possible measures of intervention to equalize the employment of members of minority groups in every sphere and level of employment, and to evenly distribute students and teachers and administrators through school districts expanded as broadly as possible to make their distribution approximate a national statistical norm." This educated opinion is based on the following ideas: that "minorities cannot make progress even if discrimination in employment is effectively outlawed, and that they can only make progress if quotas are set for them . . . that any concentration of black students and teachers in schools must be due to segregation . . . that blacks cannot learn unless they are in schools in which they approximate their proportion in some larger population ... that no blacks would live near other blacks if they could but distribute themselves evenly through a larger population" (pp. 218–19). These beliefs, though they are wrong and are held by an unidentified elite minority, Glazer says have impelled us upon the immoral and socially undesirable course he has described.

Glazer's analysis of the American consensus on ethnic policy is sound, as is his argument that it is essentially in accord with the fundamental political values of the American polity. His assessment that the policies he has described run contrary to that consensus is also sound. The problem is that the policy he describes is not the policy being followed. Ultimately Glazer's analysis is flawed by the fact that he is too cavalier in his reading of court opinions, too willing to give credence to anecdotal evidence and not willing to pay sufficient attention to the context within which judges and administrators operate. He might have done better to reflect on the difficulties involved in proving discrimination and non-discrimination. If he had combined these difficulties with (1) the shortage of time and personnel characteristic of administrative agencies, (2) the need of agencies and courts to prove that they are doing the job required of them by law, and (3) the consequent demand and need for guidelines as time-saving devices, he might have come to an alternative explanation of why statistical goals tend to be adopted and why they sometimes turn into quotas without reference to the opinion of a disembodied, misguided intellectual elite. Glazer's book deals with an important problem in contemporary American life, one which deserves and needs more careful inquiry and research than Glazer has devoted to it.

KLAUS H. HEBERLE

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The Growth of Presidential Power: A Documented History. Volume I: The Formative Years; Volume II: Decline and Resurgence; Volume III: Triumph and Reappraisal. By William M. Goldsmith. (New York: R. R. Bowker, 1974. Pp. 2342. $96.50, set.)

These volumes provide an analytic history of the development of the presidential office and powers. Goldsmith has achieved a balance of specific historical context and cumulative theme in the structuring of essays and documents. For example, we are given a picture of Polk's initiation of a war with Mexico through an essay and the key documents of the period which can be compared with a somewhat comparable situation at the time of the Gulf of Tonkin resolution. It is the author's intention that the reader be able to see both specificity and recurring theme and problem. The goal has been achieved.

Goldsmith had other objectives. He wanted all the key documents present in one book as a convenience for students. This collection of course not only provides touchstones for serious historical scholarship but at least the student can know where to begin. It is unlikely that any student or scholar will read all the documents word for word. The author's essays present the main themes covered in the documents. This raises the question of whether the reader really needs the documents as well. Should one savor the bombast of Theodore Roosevelt about presidential prerogatives in addition to Goldsmith's analysis of T.R.? There are many instances in which the documents give readers more words than they bargain for. There are letters and proclamations and political speeches that take up a lot of space. One does get the flavor of the times but it is not clear that additional insight is presented beyond what Goldsmith has already provided in his analysis. However, it is useful for the student to have key documents brought together in one place. For example, the student can see continuity in the two contradictory theories of presidential authority over the executive branch. The issues were debated in the first Congress in regard to presidential power to remove officials without congressional approval. They were again the focus of attention with the Tenure of Office Act after the Civil War and later in the Supreme Court decision of *Myers* v. *the United States* in 1926. The relevant documents are here and the student can follow the developing constitutional and political arguments.

The kinds of documents included can be characterized as follows. Volume I: State constitutions before 1789 and the Articles of Confederation; the relevant writings of Locke, Montesquieu, Blackstone, John Adams, and Alexander Hamilton in the Federalist; debates at the Constitutional convention; letters, speeches, and debates in regard to early controversies over the executive powers of the president; similar materials in regard to the presidential legislative role and the position of Commander-in-Chief and foreign policy; and finally, a section on limits to presidential power which deals with Jefferson and the Embargo. Volume II: The Whig victory in 1940 and the subsequent conflict between Clay and Tyler is captured in speeches, letters, and minutes of the congressional caucus; Tyler's and Polk's interventions in Mexico which brought war are recorded in letters, speeches, and messages to Congress; the rise of Lincoln as a national figure and his war leadership are presented; the postwar period of the weak presidency is largely described through official messages and letters except of course for the official documents of Johnson's impeachment. Volume III: Here we get more oratory as the Progressive movement and Roosevelt and Wilson take the stage and the action is carried up to Kennedy's speech at Yale on the need for modern economic thinking; the main documents of the Brownlow committee on reorganizing the presidency; instances of executive leadership—FDR assuming office, Truman on civil rights, Eisenhower at Little Rock, and Kennedy and civil rights in the South and big steel; the role of the president as world leader is depicted through the war leadership of Wilson and FDR, Truman in the Cold War, and Kennedy and the missile crisis; FDR trying to pack the Court and Truman to seize the steel mills are instances of limits on power; the "imperial presidency" is discussed with the Gulf of Tonkin resolution, the Cambodian invasion and, the secret, illegal activities of the Nixon administration and the impeachment documents.

The author's essays are the glue that holds the skeleton of documents together. Would the essays stand by themselves as a book without documents? Maybe. Some of them are very good in their originality and insight while others are simply historical descriptions known to conversant readers. However, even these are of value to new students. The sections which are especially well handled are: the origins of conceptions of the office of a single executive; the ambivalence which Jefferson showed between principle and pragmatism which Goldsmith thinks is a particular American dilemma; the Whig failure to turn a successful political coalition into an instrument of government and the corresponding weakening of the presidency in intraparty strife; Lincoln's emergence as the one man who could articulate the nature of a national dilemma better than anyone else; Franklin Roosevelt's economic failures as being due to the limited state of economic knowledge at the time; an updating of the Lawrence Chamberlain's analysis of the relative weight of presidential and congressional influence over policy, 1945 to 1964, which revises Chamberlain's findings in favor of increased presidential influence; an analysis of the origins of the containment doctrine and the question of whether Truman, Acheson, and Dulles distorted George Kennan's realpolitik argument into a moral crusade of greater rigidity; and the dialogue between Goldsmith, Arthur M. Schlesinger, Jr., and Leon Friedman about excessive presidential power in foreign and military affairs and possible remedies. Much of the essay materials on the twentieth century is description of familiar events.

The author's hypotheses about American politics might be distilled as follows: (1) Congress and president are weak together and strong together. Either-or dichotomies are inappropriate as ways of thinking about institutional power; (2) Strong, creative presidents are brought to office by national political parties and coalitions which embody the dominant trends of history, as with the Jeffersonians, Jacksonians, Republicans before the Civil War, and the Roosevelt coalition. It follows that a breakdown of the party system can only produce weak presidencies; (3) Congress can initiate policy but only presidential leadership can get it enacted; (4) Presidential power has not developed in a linear fashion but rather in cycles. However, the long run direction has been toward an increase of power because of the growing complexity of government; (5) The most effective restraints upon undue presidential power are political rather than legal. Constitutional checks are not self-enforcing; (6) The claims of presidents to constitutional authority for initiating war took a quantum leap with Korea and Vietnam. Nineteenth-century police actions which were cited as precedents were inappropriate precedents; the presidential lust for autonomy, however, was present in the earlier cases; (7) Presidents have most dramatically overreached themselves and failed when they cut themselves off from currents of opinion.

Goldsmith draws one maxim from these propositions which, in his view, will provide creative presidential leadership and prevent abuses of presidential power. That is his advocacy of the conduct of government through a "creative dialectic" of government and opposition. He would have the president and his congressional party govern in terms of a national political coalition in a balance of power and restraint, and he would require an opposition which provided constructive criticism and compose a counter-government. This is an attractive, if familiar, prescription but much of the evidence in the volumes suggests that it is not a model which can be institutionalized. The author himself points out the American ambivalence about parties in which ideology competes with interests. Nowhere does he provide concrete steps by means of which "party government" might be joined to separation of powers. He avoids the question of how his prescription can be timely in a period when the parties seem to be disintegrating. Presidential government in a federal system with separation of powers may be incompatible with party government. This leaves us with no single answer to the problem of presidential power, whether we desire strength or restraint.

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Disaster by Decree: The Supreme Court Decisions on Race and the Schools—A Sharply Critical View of the Court Rulings that Led to Forced Busing. By Lino A. Graglia. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 351. $11.50.)

As forthrightly proclaimed in its title, *Disaster by Decree* is a "sharply critical review" of the Supreme Court's school desegregation decisions. Graglia, a constitutional lawyer, devotes most of the book to traditional legal analysis of the court's opinions in virtually every major school desegregation case from *Brown vs. the Board* to *Milliken vs. Bradley. Disaster by Decree* is thus not an assessment of the impact of school desegregation policy but, rather a critique of the court's opinions in the cases underpinning that policy. This distinction needs to be made clear. For while writing good opinions is a good policy for courts to pursue, good policies do not necessarily require good opinions. For example, whether or not *Baker vs. Carr* (or for that matter *Colegrove vs. Green*) were soundly reasoned opinions has little to do with whether reapportionment was or was not a good public policy. Likewise, even if Graglia's criticisms of the court's opinions are sound, this says nothing about the wisdom of continued efforts to desegregate public education.

The author, however, intends more than a critique of the court's logic. He is obviously opposed to current school desegregation policies, although he avows support for the idea embodied in *Brown* that state-supported racial segregation is unconstitutional. The difference is that the courts are no longer simply calling for an end to segregation but requiring that black and whites actually attend school together in significant numbers. Graglia sees this shift occurring in *Green vs. New Kent County* where the Supreme Court finally rejected "freedom of choice" desegregation plans as unworkable. Graglia, in contrast to the court, views "freedom of choice plans" as racially neutral assignment procedures meeting the test of *Brown;* that they did not result in much biracial school attendance is irrelevant. For the author, the test of compliance with *Brown* is whether or not assignment procedures are racially neutral. Such a determination can be made by examining the letter of the law with no consideration given to its impact. Thus, in a residentially integrated area like New Kent County the fact that a "freedom of choice" plan resulted in very few blacks attending school with whites says nothing to Graglia about whether "freedom of choice" is really a racially neutral assignment policy. If, as the author seems to suggest, the *Brown* decision was an admonition to segregate the laws and not the schools, then it would not have been a landmark decision.

The book is weakest when the author strays from the legal analysis and, hence, from his area of expertise. Graglia repeatedly asserts, with little empirical foundation other than references to court opinions and briefs of defendant school systems, that desegregation results in white flight from the schools. In light of the availability of extensive data collected by HEW on the racial enrollments in the public schools between 1967 and 1972, reliance on court opinions and legal briefs is a grievous shortcoming. For example, in discussing the *Green* decision the author asserts that to expect preponderantly black school systems like New Kent County to achieve and maintain racially balanced schools "may be to make the wish the father to the thought" (p. 80). In fact the HEW data show that New Kent County experienced an increase in white enrollment with desegregation. The district's schools were approximately 58 percent black under freedom of choice in 1958 and approximately 50 percent black in 1972 after complete desegregation. A case-by-case, district-by-district recapitulation of the accuracy of Graglia's assertions of racial instability in desegregated schools would exceed the space limitations of this review. It suffices to say that sometimes the HEW data bears out Graglia's assertions of instability and sometimes it does not. The important point is that the author does not rely on the best available evidence.

Graglia also departs from his legal analysis to assert that the effect of school desegregation has been to "increase racial separation and hostility, to undermine support for our public schools, contribute to the deterioration of our cities, and seriously distort our political process" (p. 17). The literature dealing with the effects of desegregation is vast and complex. In support of his position the author cites only a handful of studies, most notably the controversial work of David Armour and James Coleman. Such a forceful indictment of school desegregation requires a more thorough and knowledgeable treatment of the relevant literature.

Graglia's overall negative assessment of the success of desegregation is no doubt influenced by his legal approach. Focusing only on school districts which have experienced extensive appellate litigation and which have usually been involved in Supreme Court cases, provides a rather narrow and biased sampling of the southern experience with desegregation. While some notable school districts have experienced difficulties with resegregation, desegregation has been accomplished with only limited and manageable problems throughout most of the South.

In conclusion, perhaps the author does make a point in contending that judges should not make important public policy. *Disaster by Decree* clearly establishes that sound legal training alone is not adequate preparation to evaluate public policy.

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The Shadow Government: The Government's Multi-billion-dollar Giveaway of its Decision-making Powers to Private Management Consultants, "Experts," and Think Tanks. By Daniel Guttman and Barry Willner. (New York: Pantheon, 1976. Pp. xiv + 354. $10.00.)

This study was prepared by two lawyers for Ralph Nader's Center for Study of Responsive Law. Nader's introduction argues that the government's use of the consulting industry deserves attention because outsiders cost more than government employees, operate without accountability to the public, legitimate and delay decisions, and insulate the decision maker from criticism. But the subject receives little attention from the authors. There is no further mention of *why* outsiders are used and misused. Given the amount of effort that apparently went into the study, this is most regrettable. The documentation through files, memos, and interviews is extensive, but the authors seem not to understand the internal workings of the organizations about which they write.

There are several reasons why the book fails. Foremost is its lack of coherent organization. Readers never know where they have been or where they are headed. There is not even a conclusion. Descriptions of particular tasks for which the government sought outside help comprise most of the book. Had these narrations been synthesized around a theory, say of accountability or the utilization of expertise, the authors might have found something significant to say. As it stands, readers must draw their own conclusions. And even these will be hazardous, for the authors' obvious contempt for contractors is manifest in a biased selection of examples and sources. This makes much of their report suspect. This prejudice may also underlie what might otherwise be perceived as a superficial treatment of the subject matter. For example, the authors suggest, without evidence, that contractors are more expensive than government employees. No consideration is given to their relative abilities or to the dynamics of the marketplace where the intense competition for government contracts probably results in lower costs than an agency could achieve if government overhead costs were imputed. The authors are also troubled by "sole-source" contracts, but one cannot infer that the lack of competition bothers them, since their clear preference is for in-house endeavors.

Despite its faults, the book does further our understanding of the consulting industry. The capsule histories of several management consulting firms are interesting in their portrayal of the partnerships that develop between federal agencies and business entrepreneurs. The reader concerned with think tanks, on the other hand, will find better information elsewhere. More importantly, several aspects of the contracting system are viewed critically, and although the authors' observations do not uniformly stand up to scrutiny, they are nonetheless worth noting. The authors' criticisms are randomly located throughout the book, but they could be organized as negative judgments of the client, the consultant, and the latter's techniques. There are examples of agencies using consultants to manage civil servants and as a facade to help mask previous contract overruns from Congress and the public. Not only does the government fail to maintain records on the past performances of consulting firms and individuals, but even when there is information on a previous poor performance, it is often ignored. Guttman and Willner nicely describe how agencies often hire contractors without appropriate expertise, but they extend their argument too far: they do not appear to understand that certain methodological tools can be applied across policy areas. For example, an analyst need not be a labor expert to evaluate labor policy.

The book describes how poor agency planning results in the rushed awarding of contracts at the end of the fiscal year to keep funds from reverting to the Treasury, how there are disincentives to terminate worthless contracts, how audits are either superficial or nonexistent, and how consultants are asked to advise agencies which regulate the former's clients in the private sector. Officials are severely criticized for frequently breaking procurement regulations, but the authors never consider the appropriateness of these regulations. They also fail to recognize their own inconsistencies. For example, they repeatedly complain that officials undermine competition with favoritism and sole-source contracts. In the next breath they condemn "the expenditures of relatively large sums of money for proposal preparation and bidder selection" (p. 36).

The consultant is also taken to task for often promising more than can be delivered and for sometimes shoddy work. True enough. The authors fail, though, to identify possible causes of poor work, such as limited access to agency data or time-consuming bureaucratic clearances. An unadvanced state-of-the-art is also a frequent cause, so the authors should have considered that tasks might be performed just as poorly by agency personnel. They are probably correct in assuming that the government is sometimes overcharged, but their evidence is not compelling because they seem to have little conception of the fiscal management of consulting firms. Moreover, they appear foolish when calculating a report's price per page. Also, without indicating why, they imply that it is improper for a research institute to pay thousands of dollars for academic consultants.

Finally, the consultant's tools are indicted. The authors are unfamiliar with the relevant management science or political science literatures on PERT and PPBS, but they do a good job of showing how agency officials naively commission obliging consultants to develop panaceas to the government's management problems. The authors' disdain for management techniques, however, is ironic given their crusade to reduce government inefficiency.

While many of the book's observations are trivial, irrelevant, or inaccurate, it is clear that the government's use of outside experts is sometimes ill-advised, improper, or ineffective. Reform is necessary, but *The Shadow Government* will not induce improvement both because of the aforementioned deficiencies and because of its failure to view the client/consultant relationship in its political context. Conspicuously absent is any discussion of external pressures both to use and not use outside experts. For example, the muckraking yet influential *Federal Times* monitors (through its government employee informants) how agencies use consultants and publicizes all questionable behavior. Moreover, consulting firm executives and government employees' unions lobby Congress for and against the use of outsiders whenever they feel their interest group is being short-changed.

The Shadow Government is a disappointment. Taking pot shots at relatively isolated instances of low quality work is one of the oldest and easiest games in town. In the present case, it could have been played much more skillfully and responsibly. Had the authors made the effort, they might have contributed substantially more to our understanding of the government's use of outside experts.

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The Power to Probe: A Study of Congressional Investigations. By James Hamilton. (New York: Random House, 1976. Pp. 333. $12.50.)

One might well ask: is it worthwhile to read yet another study of congressional investigations? With respect to the work of James Hamilton, the answer is a definite yes. Too often in the past, studies of congressional investigations have been written with an eye to whose ox was being gored or the proceedings of a particular committee which had run amuck like those of Senator McCarthy and the House Unamerican Activities Committee. Hamilton's analysis is balanced and fair. I hesitate to say "objective," for I agree with David Brinkley who once observed that it is probably impossible for anyone to be objective but it is possible to be fair. I take this to mean that one can, despite one's bias, examine all the available evidence, weigh it carefully, and make judgments on the basis of evidence which go counter to the bias. Again, in this context, Hamilton's is a fair appraisal of congressional investigations.

Equally important in establishing the worth of this book is the fact that recent developments growing out of the Watergate investigation have altered some of the law and conventional wisdom on crucial aspects of Congress' power to investigate. James Hamilton was in a unique position as participant-observer to see and understand the changes. As the book states: "In April 1973 he became assistant chief counsel to Senator Sam Ervin's Senate Select

Committee on Presidential Activities. In this capacity he was responsible for the committee's investigation of the Watergate break-in and cover-up, as well as the committee's major litigations, and jointly responsible for its probe of the Nixon administration's overall plan to use federal resources to reelect the President—the so-called Responsiveness Program—and the committee's investigations of certain aspects of the presidential campaigns of Senator Hubert Humphrey and Congressman Wilbur Mills." Hamilton is not a professional congressional staffer. After graduating from the Yale Law School, he was an associate with the prestigious Covington and Burling law firm in Washington for seven years prior to his service on the Ervin Committee and is now a partner in another Washington law firm. Nor does he feel compelled to justify or rationalize the actions of the committee he served. As he asserts at the outset, "the history of congressional investigations shows that the basic precepts which should govern legislative inquiries are often not followed. Indeed, we shall see certain members of the Watergate committee and its staff were not always guiltless in their conduct of the committee's investigations."

In an early chapter, "The Ervin Committee and the Courts," Hamilton explains the impact of recent court decisions on the conduct of Congressional investigations. He deals with Judge Sirica's decision ruling that the court had no inherent power to grant Special Prosecutor Cox's request that Dean and Magruder be granted immunity by the committee only if live or recorded television and radio coverage of their testimony be barred. Cox contended that there was a "very serious danger . . . of widespread, pretrial publicity which might prevent bringing to justice those guilty of serious offenses in high government office." He deals also with the "history's first congressional subpoenas to the chief executive," subpoenas seeking certain tape recordings involving conversations between the president and John Dean and other materials involving 25 others relating to criminal activities connected with the 1972 presidential election. Judge Sirica held "that Congress had provided no statutory basis for this suit and thus the court had no jurisdiction to hear it." As Hamilton explains, these decisions by Judge Sirica were not conclusive. President Nixon sought to make the best of a bad situation by seeking to appear cooperative with the committee by declaring "there would be submitted to Judge Sirica, through a statement prepared by me personally from the subpoenaed tapes, a full disclosure of everything contained in those tapes that has any bearing on Watergate." Congress then passed a bill (which became law when the president did not veto) which gave the federal courts jurisdiction over the committee's suit. The follow-up suit ended in the court of Gerhard Gesell who, as Hamilton puts it, "declined to enforce the committee's subpoenas in an opinion that is one of the most unusual in the history of relations between the courts and Congress." Basically, Gesell decided that legislative needs had to give way to the requirement for insuring fair trials. Gesell was subsequently upheld by the Court of Appeals.

The Ervin Committee was also defendant in several suits brought by Rabbi Baruch Korff, the Howard Hughes interests, and "Bebe" Rebozo, respectively. Hamilton details those suits, too.

Following his discussion of the committee and the courts, Hamilton treats in depth "The Congressional Tools"; "Investigating Criminal Conduct"; "The Informing Function"; "Investigating the Executive Branch"; "Restrictions on Investigatory Powers"; "Hearing Procedures and Individual Rights"; "Leaks"; and "Court Review of Congressional Abuses." Much of this covers old ground but it is well done, complete and in one place. Furthermore, by including the developments growing out of the activities of the Ervin Committee, Hamilton's description and analysis is as up-to-date as a publication can be.

The most compelling part of the book is the discussion of leaks from the Ervin Committee. Hamilton gives a sense of the magnitude and significance of the problem which has generally not been fully appreciated because the literature usually lacks the graphic description Hamilton gives us. He amply demonstrates the accuracy of Senator Howard Baker's the witticism: "The Ervin Committee did not invent the leak, but we elevated it to its highest art form." Hamilton concludes his chapter: "What this discussion is intended to do is galvanize thinking about the seriousness of leaks from investigating committees. During Watergate, attitudes on the committee and in the press were too cavalier. Leaks were accepted as endemic to the Washington scene—a phenomenon to be expected and not fretted about unduly. This perspective needs changing. What must be understood is that leaks often unfairly injure reputations and impair investigations, and may have severe legal repercussions that well-intentioned, law-abiding citizens should wish to avoid." Hamilton is successful; his discussion does "galvanize thinking about the seriousness of leaks from investigating committees."

In a short, pithy conclusion, Hamilton

speaks to some very important issues. He worries about current investigations into "the nation's intelligence apparatus." He asks: "Will Congress get the information it needs to perform its lawmaking, oversight and informing functions? The intelligence agencies should not be allowed to run loose, free from congressional supervision." But, he goes on: "Complicating the situation is the fact that these inquiries concern information which the executive has traditionally sought and often been allowed to protect—for example, sensitive military and foreign affairs secrets and various investigatory files." He also expresses concern about the media: "The press has manifested extreme fascination with the intelligence agencies. Unfortunately, the danger exists that some members of the media will try to pry into sensitive national security secrets by teasing leaks from disgruntled agency employees of loose-tongued congressmen and their aides. There is a possibility that a newspaper, coming upon highly sensitive facts that by any reasonable standard should remain confidential, will not act responsibly—seduced by the lure of the scoop—will rush to headlines."

Hamilton concludes his book with the observation that: "A fitting ending for this book would be to suggest how future congressional investigations can be both effective and fair, how Congress can fully do its job yet not trample on other rights that demand respect under our form of government. Unfortunately, there is no sweeping panacea." He suggests, however, "It will help if those involved in the process—Congress, staff, witnesses, counsel, the press—know the ground rules." There is no gainsaying those conclusions. For that reason, this book is a valuable contribution. Those who read it will better understand the ground rules; let us hope that, in addition to those Hamilton mentions, academicians and others who endeavor to be well-informed on public issues will be among those who read it.

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Presidential Power and Accountability: Toward a New Constitution. By Charles M. Hardin. (Chicago: University of Chicago Press, 1974. Pp. 257. $7.95, cloth; $3.95, paper.)

Crisis provokes critiques of the constitutional arrangement of powers in the American system, and proposals for large change. Among the useful analyses and calls for reform elicited by the Great Depression were Paul H. Douglas' indictment of the existing two-party system (*The Coming of a New Party* [New York: Whittlesey House, 1932]), and William Y. Elliott's prescription for fundamental adjustment of the structure of American national and state government (*The Need for Constitutional Reform* [New York: Whittlesey House, 1935]). Douglas' analysis took into account variables such as the statutory underpinning of the existing two-party system, which contemporary political scientists who predict the "dealignment" of the parties tend to ignore. Elliott's proposals, while daring and comprehensive to the point of impracticability, are still worthy of the attention of advocates of structural reform.

The subtitle of Charles Hardin's study—toward a new Constitution—states its claim to membership in this literary tradition. Strident references to Watergate in the first few pages of the book cause apprehension that the author is about to ground the need for constitutional reform on experience with one administration. But such concerns are erased as the reader is confronted by a careful, reasoned, documented examination of what Walter Lippmann called the "derangement of powers" in the national government. The lessons of Watergate, it becomes clear, merely write large that which becomes independently apparent from the cumulative force of experience of the past 40 years.

Three main lessons emerge for this reader: (1) Interest group liberalism (on which Hardin wrote insightfully and somewhat sympathetically in *The Politics of Agriculture* [Glencoe: Free Press, 1952]) is a politics of greed and backscratching which promises only disaster as a mode of policy making.

(2) Congress has failed as an institution for oversight. The legislature cannot keep the president accountable on any regularized basis, much less conduct systematic and rational oversight of the federal agencies, some of which, like Defense, have virtually co-opted Congress, with military installations and defense contracts for favored districts. And the military is present in Congress, with perhaps a third of the members of the Senate and a fourth of the House members holding military commissions and a Pentagon legislative liaison staff three-quarters as numerous as the lower house. The Pentagon has achieved a condition of unmanageability by the president or Congress. And this is a condition toward which all other agencies tend.

(3) Taking up a theme with which is prominent in Walter Lippman's *Essays in the Public Philosophy* (Boston: Little, Brown, 1955), Hardin finds that the attribution of

sovereign quality to public opinion both places upon such opinion a greater burden than it can bear and gives it too broad a scope for play. Public opinion should be channeled through the electoral process and the myth that it can or should determine interelection policy making should be quashed. Hardin seems to concede the empirical relevance of the Neustadt model of presidential power in which public prestige is essential to the president's Washington reputation and, in turn, his bargaining effectiveness. A president adroit in the manipulation of public opinion will use it to free himself of restraint. Indeed, the president's responsiveness to a sovereign public, is as conducive to his developing a sun king self-image, as is the fawning adulation of the White House palace guard, to which George Reedy attributes partial responsibility for *The Twilight of the Presidency* (New York: World, 1970).

These problems result from "deep structural faults in the American Constitution and its operational theory" (pp. 242–43) and can be addressed by change in the Constitution. The cure is to amend the Constitution to the extent necessary to institutionalize party government and an organized opposition. Both houses of Congress will be elected quadrennially with the president. From a general list of candidates for the House, enough additional congressional seats would be filled to assure the winning party a working majority. The Article I Section 6 bar to legislators' holding civil office would be removed, thus making it possible, although not mandatory, for the president to select his cabinet from the Congress. Presidential nominations would be returned to the congressional party—the nominees selected by committees composed of party members in the House and candidates from the districts (p. 184). The runner-up in the presidential campaign would be designated opposition leader and given a seat and special prerogatives in the House.

Hardin recognizes and responds to two objections to the feasibility of the recommended reforms. The first is that such structural changes must have a pre-established foundation in public attitudes and values. This he denies. History reveals that structural changes can precipitate attitudinal and value change, as well as attend it. The second argument is that the disjointed plurality of American interests and constituencies and the tendency for issues to be specific to particular locales, obviate the kind of cleavage which would support a responsible two-party system. Not so, he finds. The principal such issue in the past has been race, and for good or bad, it has been nationalized and is no longer peculiar to the South. He cites some studies and reinterprets a number of others to support his conclusion that the public increasingly perceives differences between the two parties and coherence within them. The seeming disarray of issues is reduced by the solvent of economic and social issues, to which foreign affairs might be added. Thus there is a latent foundation for a responsible two-party system.

While establishing the position of party leader and a nexus between the president and his legislative party grounded on the presidential selection process and an ultimate legislative sanction to remove and replace the president, Hardin does not offer specific comment on the reforms of party structure and process implied by these changes. Thus his focus is more common to Elliott's than Douglas'. Only a portion of his proposed constitutional changes are repeated in this review. The group of proposals does not appear to be an inseparable whole, and many of them will have strong appeal to political scientists. In view of Hardin's strong emphasis upon the baneful role of the Department of Defense in American politics and policy making, it seems obvious that an appropriate litmus test for the effectiveness of proposed constitutional change would be the likelihood of rendering DOD accountable to the president and Congress. I miss this in Hardin's book, but suggest it to those who seek to implement his program for change.

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Theoretical Perspectives on Urban Politics. By Willis D. Hawley, Michael Lipsky, Stanley B. Greenberg, J. David Greenstone, Ira Katznelson, Karen Orren, Paul E. Peterson, Martin Shefter, and Douglas Yates. (Englewood Cliffs, N.J.: Prentice-Hall, 1976. Pp. 229. $13.95.)

This book is an uneven presentation of views on several aspects of American urban political phenomena. There are eight papers and an introductory essay by Hawley and Lipsky. The topics covered include political machines, metropolitan integration, alienation and political action, leadership, institution building, and business and capitalism.

The main difficulty with the book is deciding whether the title's first word actually means "theoretical," or if it is simply tossed in to distinguish this volume from the genre of "urban reader" slapped together with scissors and paste and often bearing the wonderfully vague term "perspectives" somewhere in the title. For, whatever the term may be taken to mean—and there are several possibilities within the discipline—the fact remains that "theoretical" is a positive buzzword of the first order. Urbanists are especially likely to be attracted to a volume that beckons to us with the seductive suggestion that at last someone has got the field all nicely laid out—someone has brought order to the muddle in which we work. Sad to say, the seductive suggestion remains only that. While several of the papers are quite good, analytical, and stimulating, and while one is alone worth the cost of the book, there is no coherent set of theoretical writings here that will solve our problems. The field remains a jumbled and diffuse collection of political, economic, and sociological studies.

Hawley and Lipsky recognize this but offer the collection as examples of self-consciously theoretical treatments of time-worn yet timely urban topics. In the introduction, they offer four general reasons why the field of urban politics can rightly be considered to lack clear boundaries and agreed-upon foci, to be "adrift" and in a state of "disorientation" (pp. 2–3). First, they argue, the profession accords much greater prestige to studies of comparative national politics, international politics, and presidential and bureaucratic politics than to studies of local council members or city halls (p. 2). (I'd disagree with this only to say that there are obvious exceptions—James Q. Wilson, Edward Banfield, and Daniel Moynihan, to name just three.) Second, "contemporary behaviorally oriented political scientists often are more interested in studying relatively narrow and focused questions than in conceptualizing the larger system of which their subjects are a part" (p. 2). This seems to be a sound point, but one not limited to urbanists and certainly one not transcended by any of the papers at hand. Third, "the apparent disorientation of the field may also be accounted for by an ongoing uncertainty whether urban political systems in the United States are sufficiently autonomous to permit theorizing about them as such" (p. 3). And finally, the country's suburban settlement patterns have made both the Census Bureau's definition of "urban place" and the notion of a "central city," *qua* political community, obsolete.

The last two factors seem to me beside the point. It is not necessary to have autonomous units, geographically discrete, in order to justify the field. Obviously, nations aren't completely autonomous and the web of interdependence, whether of the nation or the municipality, is itself an object of study. So, while we may be an inchoate field, I'm not so sure that we are as much adrift as the editors imply. As they say on page 6, "The urban setting is where the themes of American justice and equity are focused and will continue to be developed most fully." That is, most people live in urban environments; these are where the institutions that produce and channel the goods, services, and practices that affect equity and justice are most fully developed; and these are the topics that matter most.

Having said this, are these papers sufficiently trenchant that they are likely to halt our drift or our disorientation? Not likely, but several are worth reading.

Martin Shefter's piece, "The Emergence of the Political Machine: An Alternative View," is an absolute gem. Shefter shows how historians have misled us concerning the origins of the machine (specifically Tammany Hall) by failing to recognize that the social characteristics of a population who support and make use of a machine have little to do with either the elite figures who developed it or the motivations that led people to create, sustain, and adapt it. Where urban historians and sociologists seem to have gone wrong was to confuse correlation with cause in the most ahistorical of ways. Simple enough, so stated, but Shefter's refreshingly original insights may help free urban analysis from social determinism. This alone is worth the cost of the book.

Five of the papers offer less scintillating though solid treatments of interesting topics. Peterson and Greenstone re-examine their five-city study of OEO programs in search of generalizations about ideology and leadership. Though the data seem very limited, they manage to find several counterintuitive generalizations. Douglas Yates examines decentralization experiences of various scopes for clues to the limits of "innovation and institution building," and Stanley Greenberg lays out an analytical structure for the examination of the connection between alienation and political action. Willis Hawley does the same for the problem of metropolitan political integration, drawing heavily on his study of the San Francisco Bay area. If I were to use this book with undergraduates, the Yates, Greenberg, and Hawley pieces seem particularly valuable as fairly complete maps of the problems discussed. The fifth paper in this group is a re-working by Michael Lipsky of his ideas on "street level bureaucrats." This version seems more remote from empirically testable theory than the earlier versions, but more complete and on the whole more interesting.

The remaining two papers are by Karen

Orren and Ira Katznelson. Orren's paper has to do with big business and inner city "communities." Katznelson's is a somewhat disjointed discussion of capitalism and systems of social control put in the context of the so-called suburban "colonial exploitation" of the city. While the dysfunctions of capitalism are certainly worth examining as factors in urban decay, it seems silly to view it in terms of colonialism rather than in terms of practices in investment, employment, and welfare.

On the whole, the papers provoke thought. The book is no salvation for an urbanist adrift, but it is worth reading and using.

RAYMOND E. OWEN

University of Pittsburgh

The Political Economy of the Space Program. By Mary A. Holman. (Palo Alto, Calif.: Pacific Books, 1974. Pp. 398. $24.95.)

With important exceptions, contemporary academic political scientists and economists have ignored political economy—the central province of the territory uneasily shared by the two disciplines. The enormity of the oversight is accentuated by the present state of both polity and economy and society's desperate need for detailed knowledge and wise counsel where politics and economics meet. Therefore, the present volume is a welcome entry into a neglected field and both NASA and Mary Holman deserve thanks for providing it.

Holman, a respected economist, has put the tools of her discipline to the task of analyzing the space program. This analysis considers such questions as national resource allocation and economic growth, regional and local social and economic effects of the fluctuations of the aerospace industry; it also considers the uses of cost-effectiveness analysis and Planning, Programming and Budgeting in NASA and Bureau of the Budget decision making and management for the space program. This useful book covers well an important topic in the depth and breadth the subject warrants.

The book is best when it applies tools of statistical and economic analysis to hard data. Examples are the discussions of regional and local impacts, economic concentration, geographic distribution and patent policy. These sections are important to scholars and policy makers alike. While elsewhere in the volume Holman is an enthusiast for the space program, her detailed analysis of the experience of the 1960s wisely leads her to conclude: "Although the space progam was a stabilizing force in the American economy in the 1960s, it is not a suitable tool of discretionary fiscal policy" (p. 267). The discussion of NASA's use of sophisticated budgeting and cost-effectiveness techniques correctly notes their contribution to NASA's successes. But the author carefully observes that "cost-effectiveness analysis makes its greatest contribution within a well-defined and quite narrow field" (p. 90).

But political economy must deal with the big, intractable and hard-to-quantify questions as well. It must provide a rational basis for assessing society's needs and wants, and, above all, it must inform the political process so as to produce wise decisions. It is in these areas that the book is flawed. Holman introduces the reader to techniques and concepts which could provide a basis for judgment on the big questions. These techniques and concepts include the social welfare function, cost-benefit analysis, opportunity cost and the GNP gap. She correctly shows that a healthy economy requires heavy public investment in research and development. But Holman identifies two of the space program's competitors for scarce tax dollars: environmental protection and programs to reduce poverty. Referring to the presence of Reverend Abernathy's Southern Christian Leadership Conference at the moon launch, the author dismisses the valid questions posed by that poignant confrontation of mule-drawn wagons and moon-bound space ships with such comments as, "it is unlikely that spending for space has diverted funds from the poor" (p. 347) and "to divert resources from the space program and from other activities that increase productivity makes no sense in the long run because it is self-defeating" (p. 348). Holman finds an "increasingly popular concern about technology assessment and the ecological and environmental effects of science and technology . . . which is . . . beginning to reach hysterical proportions" (pp. 352–53). She concludes, "it is likely that environmental interests will become a major challenge to the progress of research and development during the 1970s" (p. 353).

The case for space need not rest on arguments like these. There *is* an opportunity cost for resources spent on going to the moon, even if they come from an unused economic capacity. The cost is not dollars and productive resources alone, but the diversion of attention, talent, public concern and priority from other tasks of great importance. Economics and politics meet at the point of decision where a society expresses preferences. The political system can give primary attention only to a few first choices at a time. Space enjoyed high priority and presidential attention for a decade.

The War on Poverty barely made it to the first launching pad. If the space program had been aborted as early as the War on Poverty, it wouldn't have looked very effective either. The analytical tools of political economy permit weighing choices better than this book does. But *if,* as I seriously doubt, the issue is really space versus beating poverty or having clean air and water, I'll take solution of our urgent problems here on earth over a handful of moon rocks.

BREWSTER C. DENNY

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Public Needs and Private Behavior in Metropolitan Areas. Edited by John E. Jackson. (Cambridge, Mass.: Ballinger Publishing Co., 1975. Pp. 228. $13.50.)

Why has "urban political theory" been so slow to develop? Partly, no doubt, for the same reasons that political theory *in general* has been so slow to develop: the lack of a commonly accepted paradigm, disagreements about the questions for which answers must be sought and our slowness in developing analytic tools that might give us leverage on things political—supposing we could agree what those things were. But if all this is true for "urban political theory" there is at least one further explanation for its theoretical tardiness: the difficulty of specifying what was peculiarly *urban.* With some notable exceptions (the work of Norton Long comes to mind), efforts at theory construction failed to relate political processes to variations in the social and economic structure of the urban context. Without this, the best that could be done was endless bashing of heads against the perennial question "who governs?" and the use of metropolitan areas as a convenient data-gathering site in the search for answers to more general political questions.

This book is a clear indication that things are changing for the better. It is a collection of essays (published here for the first time) by authors from a variety of disciplines, all focusing in some way on the manner in which people go about seeking the metropolitan context they prefer and the consequences of these efforts for the metropolis in general. There is an excellent summary essay by the editor John E. Jackson, two essays around the Tiebout hypothesis of residential location, one essay about estimates of voter demand for school expenditures, one about voter demand for public services, and a concluding essay (which this reviewer found particularly interesting) about the development of municipal service equalization suits.

The interesting thing about the collection (beyond the substance of the individual papers, the quality of which is uniformly high) is the analytic paradigm that appears to be emerging. Jackson has no trouble, in his introductory essay, in spelling this paradigm out and locating each contribution in its terms; although all the papers were designed for a conference whose theme was fairly explicit, his task would almost certainly have been far more difficult a few years ago.

The terms of this paradigm include the physical, social and economic variation among metropolitan areas, the expectations that people have of their neighborhoods as places in which to live, the behaviors they indulge in while seeking to realize those expectations, and the impact of those behavior to the neighborhood conditions themselves. People have three broad ways of satisfying their demands: purchase through private markets, movement to a different residential location, and political action of various kinds. In exercising these options they are constrained in various ways, most importantly, by income (which influences their ability to manipulate markets and find more attractive residential areas through movement), race (which influences, most obviously, residential options), and the realities of political power defined by the design of political institutions. Jackson's essay is a critical evaluation of each paper in terms of the light it throws on various parts of this paradigm. As a theoretical contribution it would have been stronger had he chosen to point out the continuities between the paradigm he develops and the efforts of others in more or less the same terms (most obviously, Oliver Williams), but those continuities are clear enough. There is sufficient cause for jubilation in the fact that they do exist.

The well-known paper by economist Charles Tiebout serves as a starting-point for several of the essays contained in this volume. Bloom, Brown, and Jackson are able to show that local fiscal decisions do serve to explain some of the variance in residential location decisions, but they show also that demand making vis à vis government (or voice, to use Hirschman's term) is the more characteristic manner in which people try to attain the level of government services they desire. Similarly, Mayo shows that public expenditures (most importantly, school expenditures) are less important in location decisions than such things as housing characteristics and travel time to work. The point is further developed by Jackson's analysis of electoral motivation that provides persuasive evidence that people do use voting as a means of expressing preferences and influencing the type of public services available. Jackson rightly turns attention to evaluating how well political structures of various kinds allow people to make such determinations. To the extent that movement is rejected as a device for solving peoples' residential demands, we must examine the uses of political action, and these papers give us a useful sense of the relative importance of these two tactics.

This volume gives us warning, if such were still necessary, that scholars from other disciplines are quite capable of constructing important political theory if we do not undertake the task; only two of the authors represented are political scientists, while there are three economists, two lawyers, and one urban planner. It will not be much use as a text in introductory classes at least, but this is not its intention. It is intended as a serious and unified contribution to urban theory, and as such it succeeds very nicely indeed.

JOHN ORBELL

University of Oregon

Public Policy Making in a Federal System. Edited by Charles O. Jones and Robert D. Thomas. (Beverly Hills, Calif.: Sage Publications, 1976. Pp. 288. $17.50, cloth; $7.50, paper.)

Readers of this volume will find neither a general treatise on the theory and functioning of federal systems, nor studies of intergovernmental politics such as Donald Haider's *When Governments Come to Washington* (Free Press, 1974), nor anything close to a definitive examination of particular governmental programs or policy areas.

What is offered in this collection (many of which were originally presented as conference papers) is an exploration of problems of policy evaluation and policy implementation generally in an intergovernmental (essentially American, national-state) context. Part I of the book offers an overview of evaluation and implementation problems. The other three parts focus more closely on how these problems are reflected in national revenue sharing, environmental and social-remedial policies.

Policy evaluation requires the selection of some criteria by which policy impacts, effects and outcomes can be judged. The criteria may simply be political, i.e., whether or not a policy achieves the support of the relevant public or, more negatively, is not intensely resisted. Policy evaluation is also often taken to require a statement of policy goals clear enough to provide a standard by which a policy's impact can be measured.

Many policies, of course, fail to provide clearly stated or agreed-upon goals by which progress can be measured. Revenue sharing has been a case in point. Wisely, the Brookings Institution's five-year study of revenue sharing did not attempt to measure performance against goals, but focused on the distributional, fiscal and political effects of the policy. Policy issues raised in the debate over adopting the new program provided the criteria for evaluating the actual impact of the program. This approach and problems in implementing it are briefly discussed by Richard P. Nathan in chapter 3. (This article is based on Richard P. Nathan, Allen D. Maxwell, and Susannah E. Calkins, *Monitoring Revenue Sharing,* Brookings, 1975).

Policy impact and evaluation studies not only encounter a host of methodological and technical problems (many of which are discussed in this volume) but problems related to deliberate bias and the influence of such studies on the political process. Deliberate bias, as Bruce A. Rocheleau suggests (chapter 11), can be expected when organizations attempt to evaluate their own programs. Likewise, one might be justly skeptical of the evaluations offered by nongovernmental groups with some stake in the policy being evaluated. Outside groups, who may be expected to be objective (i.e., at least free from intentional biases), may lack human and fiscal resources to accomplish their research. Even the best of impact and evaluation studies, moreover, may not affect the political process. As Nathan speculates, "once enacted, the political forces created to continue a revenue-sharing program in essentially its present form may be so strong as to preclude important changes" (p. 77). The recent extension of this program appears to confirm this hypothesis.

While some authors in this volume are concerned with general problems of policy impact and evaluation, others focus more narrowly on policy implementation. These chapters are worth reading. Of particular interest is Robert D. Thomas's discussion (chapter 6) of the problems encountered by federal administrators implementing national air and water pollution policies. Thomas views Environmental Protection Agency administrators as under pressure from Congress to implement national objectives but also forced to rely on the cooperation of state and local officials who may be unable or unwilling to cooperate. The EPA has the power to assume pollution control activities, should the states fail. Yet, as Thomas argues, the EPA "is no better equipped with adequate resources (funds and personnel) than the states and is certainly ill-equipped to deal politically with either governmental or private polluters in the states. If EPA officials had to carry out pollution control and abatement for a state, it would slow down activities both in the state and nationally, because EPA officials would have to put aside their own work to do the state's work" (p. 131). Given these and related conditions, "states are afforded the opportunity to bargain on the application of national objectives and negotiate for their definition of how these objectives should be applied. As such, they are able to tailor the implementation of national objectives to fit their perceptions of specific geographical and political circumstances" (p. 132).

One might expect a book devoted to policy making in the federal system to contain more articles of this nature and to explore such subjects as state and local contributions to Congressional policy, state-local relations, and the pattern of horizontal federalism. This volume is perhaps more valuable for what its authors have to say about public policy generally than for what it says regarding public policy making in the federal system.

DAVID R. BERMAN

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Lyndon Johnson and the American Dream. By Doris Kearns. (New York: Harper and Row, 1976. Pp. xii + 432. $12.50.)

Only rarely does a psychobiographer have the access to a major political figure that Doris Kearns has had. Though Professor Kearns first got to know Lyndon Johnson as a White House aide in the spring of 1968, her closest contact with him came at his ranch in Texas during the last four years of his life. She was not only able to observe Johnson at close hand, but also to hear his own explanations of his political career, his recollections of his childhood, and even his accounts of some of his dreams.

Kearns is much too sophisticated to accept everything Johnson told her at face value. She takes account of his tendency to exaggerage and to tell each person what he or she might want to hear. She is aware that much of what he told her were obvious attempts to justify his presidency. But Kearns also understands that even self-serving or manipulative disclosures are apt to reveal psychic characteristics, which appear in the selection of themes and details despite attempts at self-censorship. Moreover, much of what Johnson told her about his childhood and his dreams does not fit into any self-serving pattern, but shows him to be a vulnerable human being. Many of his most revealing revelations came in the last two years of his life when he suffered angina pains and was apparently aware that he was dying.

Kearns' explanation of why Johnson talked so openly to her is quite plausible. At one point Johnson told her that she reminded him of his mother, but she thinks that he was more likely reaching out for some form of self-understanding at this time in his life, and she was simply there to hear him out. One of the dreams he reported to her suggests why Johnson may have chosen her rather than one of his male political aides for his self-revelations (though Kearns does not make this point). The dream reveals his fear that his relationships with these aides were based on their desire to share his power, rather than their affection for him. In this dream he was lying paralyzed in the Red Room in the White House, unable to act or speak as he heard his aides in the adjoining room "squabbling over who would get what part of his power . . . and not a single aide tried to protect him" (p. 33).

Kearns offers no startling new interpretations of Johnson's political style or adult personality (except to note certain paranoiac tendencies in 1968). But she does trace his strong power drive, his confidence in his manipulative skills (and some related conflicts and defenses) to a powerful oedipal conflict, thereby lending a cohesion to a complex set of personality traits which might otherwise seem unconnected.

The picture of Johnson's childhood that emerges in Kearns' book is quite different from his early public descriptions of his "rags to riches rise from a happy childhood, guided by an adoring mother and the example of a manly, principled father" (p. 17). Johnson recalls his mother, Rebekah, as a sensitive woman who could have been a "great novelist" (p. 21) had she not lost her confidence after the death of her father and married the rough, drinking, vulgar-talking Sam Johnson. She was disappointed in her husband, and her eldest son—the object of her intense adoration, attention, and dreams—was to make up for that disappointment. This kind of material love, as Freud (and Kearns) point out, is apt to instill in its object the faith of the conqueror—a unique self-confidence that will sustain him in larger enterprises later on.

But this kind of love also has its dangers. Rebekah evidently encouraged emotional intimacies with her son that could have fanned a destructive, guilty competition with his father. When Lyndon failed to meet her standards she acted as if he did not exist. At the age of seven or eight, for example, Lyndon refused to continue his music and dancing lessons. "For days after I quit those lessons she walked around the house pretending I was dead. And then to make it worse, I had to watch her being especially warm and nice to my father and sisters" (p. 25).

The price Lyndon would have had to pay for her complete approval would have been the rejection not only of his father, but also of his own masculine identity. But young Johnson had the ego strength to refuse that alternative. Instead he learned to walk a fine line between his parents. He adopted the rough, masculine ways and the political profession of his father; but unlike his "daddy," he would achieve success and an ultimate capacity to do good for others.

In short, Johnson's confidence in his manipulative skills and his feeling of being a conqueror served him well in his later political career. But the conditional love he had received also connected his own well-being with the approval of others, so he turned his considerable skills toward mastery of the external world, always seeking what he had lacked in childhood. "The problem was that each successful performance led only to the need for more. There was no place to rest so long as love and the self-esteem based on love depended upon another's approbations" (p. 45). Thus he sought the approval he needed in elections (although Kearns overemphasizes the psychological nature of several physical ailments at such times). His insecurities were manifest in his fear of being alone—he could not stand to be by himself ("not for an evening or for an hour" [p. 8])—and in his need to control every aspect of his environment (see his compulsion to control his staff as president, pp. 238–45). Even one critic he could not handle could destabilize him in a public speaking situation.

At some deep level, as Kearns points out, Johnson equated manners and gentility with passivity and feminity, values which he could not use to model his own behavior but which, like his mother, still had strong hold over him. His attitudes toward the Kennedy intellectuals and the authority he gave them in foreign policy matters, where he felt his own lack of expertise, partially manifested this ambivalence. Johnson's passionate hatred of Robert Kennedy suggests that he was severely threatened by Kennedy's successful syncretism of toughness and intellectuality, masculine and feminine qualities. And his difficulties during his retirement in working on his memoirs were also based on this same deep ambivalence: he felt he should be genteel and refined and contemplative, but could not because these qualities were effeminate and not Lyndon Johnson. (Kearns fails to mention in this context another factor which she notes elsewhere in her book and which may have helped create this problem—i.e., a realization at some deep level that he had failed in some basic presidential goals.)

Despite her emphasis on Johnson's motives and style, Kearns is no simple reductionist. She is quite aware that Johnson's personality alone did not determine such policies as his decision to escalate the Vietnam war. Further, she points out, there is a subtle interaction between political institutions and the personality characteristics that may be rewarded in each of them. Johnson might not have succeeded so well in other political situations and contexts. Moreover, Johnson's skill in bending the political milieu to serve his own purposes cannot be fully explained in terms of his childhood conflicts. He was an intelligent, energetic man who learned how to exercise power in several later contexts.

Kearns also shows how Johnson's perceptions of the world influenced his policy choices. With his simple faith in the American creed he saw no need for basic changes in American institutions or beliefs. He imagined himself to be free of ideology. "I am . . . a free man, a U.S. Senator, and a Democrat, in that order. I am also a liberal, a conservative, a Texan, a taxpayer, a rancher, a businessman, a consumer, a parent, a voter, and not as young as I used to be nor as old as I expect to be—and I am all these things in no fixed order. . . . At the heart of my own beliefs is a rebellion against this very process of classifying, labeling and filing Americans under headings" (p. 156). With a better delivery of governmental services, and a better environment, others would achieve as he had.

The power of the Munich metaphor is evident in his early decision to escalate the Vietnamese War, despite its inapplicability to a civil conflict. His notion that war could be uprooted by eliminating poverty, his tendency to equate the American way of life with the cause of civilization, and his faith in the ability of diverse peoples to reach agreements through reason all led him to misunderstand fundamentally the Vietnamese people and how to deal with them.

Kearns' book contains some editorial errors. In one place the Pedernales is mistakenly located in "East Texas" (p. 28), though Kearns is clearly aware of its location in western Texas.

But there is one major failing in the book. Kearns could have given some of her interpretations more authority and avoided at least one important misinterpretation if she had checked her observations with others. She clearly overestimated the extent to which Johnson, as majority leader, reduced once powerful senators to "lieutenants in [his] system" (p. 110). Her suspicions that Eastland may not have urged Johnson to run for the presidency in the summer of 1968 (contrary to Johnson's recollections) could have been settled one way or the other had she contacted Eastland. One also wonders who else observed the paranoiac trends and the fragmentation of personality that Kearns noticed in Johnson's final days as president and afterwards at the ranch.

The postscript to this book is particularly valuable as a subtle summary of the interplay between culture, institutions, office, and personality, as manifested in Johnson's career. But it also contains a powerful warning about how power is exercised under the imperial presidency.

As Kearns points out, Johnson showed us that the resources of the presidency could be used to conceal "much of the exercise of power; that presidential authority could be exerted on the basis of undisclosed information and the private interpretation of information; indeed, that in many cases even presidential actions and decisions could be concealed" (p. 397). Further, the Johnson presidency suggests that the fundamental restraints in the American political system are not to be found in the constitutional checks and balances but in the media and public opinion. Johnson's career provides further evidence that the basic qualities of a leader do not change "when he assumes new and larger responsibilities" (p. 398). The basic lines of character are deeply engrained and do not easily change. Finally, she warns that the "exaggeration of the President's personal powers (both self-induced and media-propelled) is an inevitable source of frustration as the President's actions invariably fall short of expectations producing a destructive cycle for the man, the office, and the nation" (p. 400).

BETTY GLAD

University of Illinois

The Party's Choice. By William R. Keech and Donald R. Matthews. (Washington, D.C.: The Brookings Institution, 1976. Pp. xii + 258. $9.95, cloth; $3.95, paper.)

This is the concluding volume of the Brookings series entitled *Studies in Presidential Selection,* six volumes of which have previously appeared. The argument and essential contents of the present volume were previewed in a chapter by Donald Matthews in *Choosing the President,* edited by James David Barber and published by Prentice-Hall for the American Assembly in 1974.

The book is a concise treatment that nonetheless manages to cover an enormous amount of ground. It is organized in chapters that parallel the authors' main analytical scheme: the differences between the "in" party and the "out" party and the differences between three major stages of the nominating process—the three years from a presidential election to the beginning of the next presidential election year; the formal nominating process from the first presidential primary to the conventions; and the final action in the conventions. Chapters 2 and 3 deal respectively with the "in" party and the "out" party during the three-year period. Chapters 4 and 5 deal with the "in" party and the "out" party during the campaigns before the conventions. Chapter 6 deals with the conventions, where the distinction between "in" party and "out" party is found to be less important. Within each of these chapters, the authors develop categories of patterns of action; and in each category they supply a set of capsulized case histories of the experience in chronological order.

The experience treated in this analytical scheme begins in 1936, when the Democrats abandoned the two-thirds rule, the Gallup poll was first published, and the Literary Digest poll died. This gives the authors a total of 20 major party nominations for study; and the three stages of each are reported in their appropriate chapters. The result is probably a much more balanced view of the variety of the nominating process that can be obtained in any other way. It also demonstrates the importance of the early period, which has been decisive more often than not: "Thirteen of the twenty nominations in the last ten elections were clearly identified by the beginning of the nominating year. . . . Two more contests were structured as two-candidate races before the first primaries. . . . In only five of the twenty nominations was the preprimary prognosis misleading or unenlightening about the likely winner. . ." (p. 227). By contrast, the frequent unimportance of—or in any event the lack of change resulting from—the primaries is also emphasized: "Only once were the primaries instrumental in the demise of a candidate who was likely to win nomination before they started. Only once were they instrumental in the emergence of a nominee who was otherwise unlikely to win nomination. Both events occurred in 1972 when McGovern was nominated by the Democrats" (p. 229).

The book thus treats a body of experience—the lessons of which were disproved by the Democrats in 1972 and again in 1976. This raises the major problem for the future of the book: Has it been published just at the point when the future of the nominating process can no longer be predicted from its past? The authors make it clear that a proliferation of primary elections was one of the factors in the unique outcome of 1972, although McGovern's style of campaigning in the caucus states was equally credited for his nominating victory. The primaries proliferated even further in 1976, and there is no sign that they are going to decrease.

The authors are cautious in dealing with all questions of reform, as might be expected from their approach. They do not see much that can be done about the early, informal nominating stage, despite their estimate of its importance. They credit the primaries, "despite all their blemishes," with being "the most effective of the existing means of involving the populace in the presidential nominating system" (p. 231). They oppose a national presidential primary, but present an even-handed treatment of pros and cons. They favor the movement toward grouping the existing state primaries on a limited number of dates, a movement that may be gaining some momentum in Congress. They have few suggestions for any change in convention procedures, although they would prefer to see the vice-presidential nomination held over for action after the convention by "a representative national party committee" (p. 248), much like the confirmation process for Sargent Shriver when Thomas Eagleton was replaced in 1972.

The problematical aspects of the contemporary situation obviously limit all attempts at comprehensive analysis and prediction of the presidential nominating process. As the authors testify and this reviewer can agree, it is a complex, ambiguous process and capable of the most unexpected developments. Within such limitations, the authors have produced what is probably the best available current treatment of their subject, and one that will be an important point of reference for some considerable number of years to come.

PAUL T. DAVID

University of Virginia

Pursuing the American Dream: White Ethnics and the New Populism. By Richard Krickus. (Bloomington: Indiana University Press, 1976. Pp. xv + 424). $12.50, cloth; $3.95, paper.)

This book covers topics treated in Michael Novak's *The Rise of the Unmeltable Ethics,* albeit with less self-righteousness but also with less sophistication. Once more the white ethnics are portrayed as the forgotten, misunderstood people, their interests neglected by government, their values and sensibilities violated by liberals and leftists. Taking a cue from the excellent work done by investigators like Andrew Levison and Richard Hamilton, the author argues that the ethnics (a) are not economically well-off and (b) are *less* conservative on most issues than upper income groups. Like others before him, Krickus chastises the left activists and McGovernites who wrote off the white ethnics as hard-hat reactionaries. He believes we are entering a period of reform, a "New Populism," whose success depends in large part on the reformers' ability to reach the ethnics.

Krickus recounts the ethnic experience, beginning with the well-worn story of the migrations and the problems faced in the New World by the white immigrants and their children. A refreshing chapter on the "working-class legacy" treats the ethnics' militant role in the early industrial conflicts and casts doubt on the notion that immigrants were primarily a conservative influence in labor struggles. This is followed by a discussion of "white ethnic politics," which is more accurately an account of ethnic electoral behavior. In this book, as in so many others, contemporary ethnic political consciousness is treated as something almost entirely encompassed in the act of voting.

The urban political machine is depicted as a benign human service organization which, for all its inefficiencies and favoritisms, was one of the few institutions that supposedly cared for the ethnics. In reality, the machine operated on a highly regressive, corrupt and exploitative system of taxation and influence peddling which wreaked havoc on the working class. The machine served the slumlords, sweatshop owners, racketeers, big franchise holders and business firms. Krickus is not the first to be so impressed by the clambakes and Christmas baskets as to overlook the *systemic* role the machine played in deflecting and muting working-class grievances while integrating the ethnic populace into politico-economic relations that consistently violated their material interests. Although the author offers a passing critique of the pluralist view and takes a few gingerly swipes at corporate greed, his own lack of a theoretical framework extending beyond the pluralist model causes him to fall back on the same standardized images of ethnic political life

Like so many others, Krickus descends to utter confusion and ax-grinding when writing about the antiwar movement of the 1960s, the New Left, radical socialists, and McGovernite New Politics—all of which he tends to lump together. He incorrectly identifies Norman Mailer, Charles Reich and John Kenneth Galbraith as the 1960s "radical left's" intellectual gurus. He labels Daniel Bell "a left-wing intellectual." He characterizes leftists as people found "attending meetings of SANE, ADA, Common Cause and ACLU and active in other do-gooder causes." But he does not explain what's so terrible about do-gooder causes. He conjures the radical chic stereotype of activists, whether "moderates" or "radical-left," as persons sensitive to "the latest utterances fashionable in cosmopolitan circles." Once more the protestors are made the issue rather than the things they are protesting. Krickus' cartoon image of the left is greatly indebted to the major publications, periodicals and mass media, yet incredibly enough, he asserts that the radical movement of the 1960s enjoyed a "near-domination of the institutions which communicate news and disseminate information." Not even Spiro Agnew went that far.

The Kennedys and others who fit the author's centrist political biases are declared to be "sensible" and "pragmatic," and are spared the ad hominem attacks delivered upon everyone to the left of them. One of the more unforgivable sins committed by the radicals (including McGovernites), according to the author, was to be concerned with "moral issues" like Vietnam, issues which the simple patriotic white ethnic folk supposedly could not appreciate. (There is more than a little patronization shown toward the ethnics by those who elect to speak on their behalf.)

In Krickus' book the left is blamed for everything: thus, the white ethnics' discontent is "attributable to the left's failure to pay attention to their problems." Less agile minds might see the ethnics' discontent as caused mostly by the problems themselves—the demoralizing hardships of underemployment, inflation, low wages, high taxes, job-connected disabilities, staggering medical and housing costs, etc. Contrary to the impression one gets from much of the literature, ethnics do not spend their lives worrying about their acculturation-identity crises nor even about their relations with left liberals. They are primarily preoccupied with trying to achieve and maintain a decent material existence for themselves and their children in corporate America.

The ethnic experience, then, as people like William F. Whyte and Herbert Gans noted decades ago, is in large part the experience of the urban working class, be it Anglo-Protestant, black, Chicano or whatever, Krickus knows this and even says it, but he does not pursue any systematic investigation of the relationship between ethnicity and class.

The ethnics in America have been robbed of something more than their identities; indeed, in many cases (consider the millions of low-income blacks), their identities are about the only thing they have been allowed to keep. What has been stolen from so many of them is their labor, their health, their communities and their ability to live with any margin of ease and security. The thieves and exploiters have been not only WASPs but also some fellow ethnics.

There is a need for a book which draws the theoretical linkages between the ethnic experience and those larger historical, class and systemic forces of corporate America which have operated with such distorting effects on ethnic life. How does ethnicity define, mute or activate class experience? How it is, in turn, intensified or diminished by class experience? How does it operate as a class control factor? How does it serve intra-class competition and inter-class struggle? How is it sometimes functional and sometimes troublesome to the existing system of power and property? Without a treatment of these questions and an understanding of the wider societal linkages, ethnic consciousness is reduced to a set of attitudes and grievances which we can commend or condemn but which we have not really explained. For all his compassionate defense of the white ethnics, I regret to say, Krickus has not deepened the informed reader's understanding of the ethnics' struggle nor of the society that has brought them to their present impasse.

MICHAEL PARENTI

The Divided Academy: Professors and Politics. By Everett Carll Ladd, Jr. and Seymour Martin Lipset. (New York: McGraw-Hill, 1975. Pp. xviii + 407. $17.50.)

The major work at hand is hardly the first or the latest of the authors' publications on the politics of the American academic community.

Most of us must already have read at least one of the monographs or articles that Ladd and Lipset have produced, in an apparently continuing stream, from surveys of professorial opinion. Indeed, the authors are the established contemporary chroniclers of faculty attitudes about a variety of campus and off-campus issues. Here, in *The Divided Academy*, Ladd and Lipset present most fully their findings from the massive Carnegie Commission survey of 1969–a mail survey that returned 60,000 professorial questionnaires, each containing over 300 items of information. The large numbers enable the authors not only to generalize with considerable confidence about American faculty members, totaling 600,000 but also to analyze disciplinary and other academic subgroups which would be insufficiently represented in national samples of the usual limited scale. Although relying mainly on the 1969 survey, Ladd and Lipset also cite data from a much smaller 1972 faculty survey that they had designed to update certain findings and particularly to tell us about professorial voting preferences in the 1972 presidential election. Altogether the data are impressive, and so are the analyses, the historical interpretations, and the ideological speculations.

Making good use of the opportunity to analyze faculty subgroups, the authors find differences both so numerous and so significant as to establish the theme expressed in the book's title. Among the divisions discovered within the academic community, several are especially striking if not entirely surprising. Disciplinary differences certainly stand out. For example, 86 percent in clinical psychology, 85 percent in anthropology, and 79 percent in history voted for Humphrey in 1968, while, at the reverse end of the scale, only 28 percent in civil engineering, 33 percent in mechanical engineering, and 36 percent in agriculture so voted (p. 368). Roughly similar interdisciplinary differences are reported from other measures of left-right and liberal-conservative preferences. Even within the social sciences, whose faculty members are generally to the left of their colleagues in other fields, there are noteworthy differences between disciplines. In particular, the "behavioral" fields of sociology, anthropology, and psychology are to the left of the two "institutional" fields of economics and political science (p. 114). Still it is the wider gulf illustrated by the difference between social science and engineering that is the principal basis for the authors' reluctance to stress the politics of the academic community as a whole.

Their reluctance on this score is bolstered by discovered differences between other-than-disciplinary subgroups. Younger faculty are more liberal than older faculty, and Jews more so than non-Jews. Of special interest to the authors and probably to many of us is that professors of high status and achievement, commonly belonging to the "research culture" of prestige universities, are associated with more liberal politics and a more intellectually critical view of the established order than are professors in the "teaching culture" of non-prestige positions or institutions (pp. 144–45). In line with this finding is another that also confirms earlier work: among faculty members, consultants for the federal government are more likely than nonconsultants to be critical of government policies. "The academy," Ladd and Lipset remark, "turns the class theory of politics on its head" (p. 148).

The divisions within the academy are at the center of the authors' attention throughout the book, and not just in the half of the book explicitly devoted to exploring these divisions. Yet Ladd and Lipset are conveying something at least as important when they present their well-supported findings, from several measures, that American professors have left-of-center political preferences substantially more often than do American citizens generally. For instance, 56 percent of the academics, compared with 38 percent of all American voters, cast their 1972 presidential ballots for George McGovern (p. 222). And 46 percent of faculty members (in 1969) characterized themselves as left or liberal, compared with only 20 percent of the U.S. public (in a 1970 Gallup poll). These general tendencies of the academic community seem of greater consequence in the larger political system than do the differences between faculty subgroups. Intriguing though such differences are, they may matter less than the collective impact of a decidedly left-of-center academic establishment. The impact need not be conceived primarily as classroom influence on students; it is by no means clear that many professors exert, or would be able to exert, political influence through their classrooms. What appears more significant is the off-campus participation of faculty members in public affiars–whether through cultural discourse, governmental involvement, or electoral campaigning. Professors are numerous enough, and their resources of time and talent are sufficient, to constitute a significant political force. Ladd and Lipset are certainly right to take this potential into account when, in their last chapter, they discuss how professors, along with ideologically similar intellectuals outside the universities, may function as critics fostering social change.

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The Politics of Housing in Older Urban Areas. Edited by Robert E. Mendelson and Michael A. Quinn. (New York: Praeger, 1976. Pp. xxii + 261. $20.00.)

Mendelson and Quinn have put together a useful and instructive book of readings focusing loosely on the problems of housing in America's urban centers. Only recently have these problems been perceived as important and worthy of inclusion on the national policy agenda. Instead housing policy has focused on the need for either new construction or clearance (implying destruction) of existing neighborhoods. Robert Mendelson's article, "The More We Build, the More We Waste: Housing in Older Urban Regions," presents an excellent description and analysis of the consequences of the pursuit of these goals by federal housing programs.

Two of the articles deserve particular attention because of the contrast they provide to each other. Michael Quinn's article, "Much Sound and Little Fury: The Place of Rhetoric in the Politics of Housing," is counterposed to Brian Boyer's "Bad Dreams in the Night: The American Housing Con Game," a fine example of the genre Quinn criticizes. Quinn argues that the inflammatory rhetoric which characterizes housing policy debates acts to prevent incremental improvements in housing conditions, and that only through the accretion of these incremental improvements is real change possible. Quinn writes that "rhetoric has an emotional logic that resists cold calculation and measured response. Confrontations in the housing policy arena frequently assume the form of a morality play in which the forces of good and evil are locked in bitter struggles. . . . In situations of this sort, accommodation is very difficult because the moral principles projected by adversaries do not lend themselves to compromise" (pp. 57–58).

Quinn's criticism of the politics of rhetoric is convincing, although his faith in the efficacy of incremental solutions is less so. The real cost of overblown rhetoric in housing is not the difficulty of achieving incremental change, but the failure to undertake the necessary analysis of the problems and their causes, a failure which leads to simplistic and inadequate solutions. Thus "we must do something about the housing crisis," the clarion call of concern, begs the question of exactly what the housing crisis is. Is it a housing shortage (probably the most prevalent—and least defensible—view); is it the structural condition of the existing housing stock (which has improved steadily if not spectacularly, over the last several decades); or is it the cost of housing (the percentage of family income spent on shelter has been steadily increasing)? Or, perhaps the prime problem is the inequitable distribution of housing opportunity by race or income class or the distribution between central city, suburb, and rural area.

The way in which one defines the housing problem ought to determine the kinds of programs designed to solve it; a program or set of programs may adequately solve one of these various interpretations of "the housing problem," while it may not improve—or may even adversely affect—one of the others. Consequently, our housing and policies are frequently vague, confusing, or even contradictory. Lacking clear and consistent goals and objectives, it is difficult even to judge whether the programs have been successful.

Boyer's article represents an excellent example of the pitfalls of rhetoric in housing. The housing problem exists, he argues, because there is a conspiracy being conducted by evil men who act against the public good. As Boyer describes it, "I finally became aroused enough to write a book entitled *Cities Destroyed for Cash*, which describes in grim detail the largest, most profitable criminal conspiracy in American history. How did the scandal get started? Quite simply, our elected representatives were duped by the powerful Washington housing lobby that is headed by the guys in the home loan business; . . . the housing boys sat down together, carved the turf, and emerged with a set of programs that would make them all filthy rich. . . . The vultures feast on the innocent and poor while lawmakers sleep" (pp. 76–77).

The analysis purges the soul, inflames the heart, and numbs the brain. It is not, of course, untrue nor is it particularly surprising that government housing policy provides substantial benefits to those in the housing industry. Government policy works through the private market, attempting to serve public purposes through structuring incentives which will encourage private entities to work in the public interest. Rather than a hidden conspiracy this represents a conscious public policy decision. However, merely ridding current public policy of the opprobrium of a criminal conspiracy does not make it successful. Pursuing the public interest through structuring incentives in private markets will succeed only if (1) there exists a clear conception of the objectives to be pursued, and (2) the structure of the private markets and the likely behavioral effects of restructuring incentives are well understood. Neither of these conditions presently exists.

Reading Charles Liebert's "The Role of the Middleman in the Housing Market" in conjunction with Boyer's attack on housing middlemen is instructive. Liebert's article is a fascinating description of his own role in housing development and management over a 25-year period. He directly confronts Boyer's thesis, observing "much undeserved blame has been heaped on middlemen. After all, they don't devise programs, but operate squarely within the system created and established by others. Once limits of the game are set, the competent intermediary is opportunistic and enterprising in achieving maximum rewards" (p. 119). Liebert's article also offers eloquent testimony to the difficulty inherent in attempting to pursue public objectives through structuring incentives in the private market. He ends his essay by noting, "I have learned that it is much easier to provide housing for the wealthy than it is for the poor" (p. 119). Unfortunately this succinct conclusion is also probably the most apt summation of several decades of federal housing policy.

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Leadership and Change: The New Politics and the American Electorate. By Warren E. Miller and Teresa E. Levitin. (Cambridge, Mass.: Winthrop, 1976. Pp. xviii + 267. $10.00, cloth. $6.95, paper.)

Everybody's doing it. This is the fourth major study of American voting behavior to appear in the last two years. Like the others—by Asher, by Nie, Verba, and Petrocik, and by myself—it constitutes an extensive revision or reversal of the findings of the classic work in the field, *The American Voter.*

All of these recent works rely on the impressive data base of the Center for Political Studies, headed by Miller himself. Given the same data, it is a conforting testament to the intellectual integrity of the subfield that the various authors come to similar conclusions. There is now general agreement that issue preferences have become a vital influence on electoral choices, and that party identification is no longer immutable, unreasoning, or fully satisfactory as an explanatory variable. More recent researchers now emphasize the variability in the vote and its dependence on particular events, candidates, and other political stimuli.

Miller and Levitin share in this consensus, but they have a unique perspective and make distinct contributions. Their book begins with a clear and brief review of the previous literature and methodology in the field, which should become the basic introduction to all courses on voting behavior. Their own temporal focus is on the most recent elections of 1970, 1972, and 1974. Their research focus is on the issues collectively known as "the new politics," or more specifically, attitudes toward four new political objects: the counterculture, political protest, law and order, and agents of social control (the military, police, and whites).

Using 1970 attitudes, Miller and Levitin combine responses to define two polar groups. The Silent Minority (*not* Majority) rejects the counterculture and political protest, while favoring law and order and social control. New Liberals have precisely the opposite combination of opinions. Most respondents, holding mixed attitudes, are collapsed into a largely undifferentiated Center. Placement in these groups is independent of party identification, but is significantly related to attitudes on specific issues such as Vietnam and race, to ideological self-identification, and to some demographic factors such as youth, education, and urban residence.

A number of surprising conclusions result. Contrary to popular journalism, the Silent Minority declined, from 17 percent of the total electorate in 1970 to 7 percent in 1972, while the New Liberals grew from 14 to 25 percent, making a Democratic victory conceivable. Contrary to the explanations of radicals, Nixon's election resulted not from the reactionary character of the American public, but from the weaknesses of his opponent: "McGovern lost, and lost as decisively as he did, primarily because he was unable to transform potential support into actual support, not because the potential was lacking" (p. 122). The South Dakotan generally failed even among his natural constituents, the New Liberals, beyond those votes which can be explained on the basis of partisanship alone. He was catastrophically rejected among those in the Center and Silent Minority. Using varieties of new analytic techniques to ascertain the normal vote, voter-candidate proximities, issue preferences, and candidate evaluations, Miller and Levitin repetitiously heap scholarly opprobrium on the Democratic candidate.

Leadership and Change is impressive for its variety of methods, its attention to the electoral context, and its consideration of alternative explanations of voting behavior. There are a number of problems. Some are minor, such as the inadequate editing which results in the mislabeling of an important table (p. 142) or discrepancies between the text and table figures (p. 86). Some are questions of interpretation. Miller and Levitin see New Politics sentiments as the basis of a new ideological division and possible party realignment in the nation. Yet their own calculations show that the only issues which are related to ideology in all age groups are economic and welfare questions, such as those emphasized in the 1976 contest between Carter and Ford. This volume may therefore only be a period piece, describing a brief interlude in American politics in which attention was diverted to passing issues of the legalization of marijuana or campus protest.

The most important question is the basic methodology. The polar groups are defined by dichotomizing each of the four sets of attitudes. On some of these questions, however, opinion in 1970 was so skewed that the bifurcating points were unrelated to political reality. Thus, respondents who "rated" the counterculture higher than 10 on a 100-point thermometer were considered relatively favorable to marijuana users, rock music celebrants, and radicals. Statistically, one can do anything, of course, but a rating of, say, 25 hardly indicates acceptance of the counterculture. Such extreme distributions of attitudes may explain, and undermine, the substantive conclusions. The decline in the Silent Minority may well result from a change in attitudes no more substantively significant than that from extreme hatred to controlled hatred. Similarly, McGovern's failure to win his "natural constituency" is not so surprising when that constituency is operationally defined as including persons very cool to the attitudes with which he was identified.

The authors' conclusions about trends and influences are still probably valid and are well-defended, but the specific results must therefore be treated with caution. However, their conclusion, the need for creative political leadership to bring public awareness of issues, deserves underlining. Their emphasis on this need brings Miller and Levitin credit as they join the scholarly tradition of V. O. Key and his defense of "the responsible electorate."

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Pragmatic Illusions: The Presidential Politics of John F. Kennedy. By Bruce Miroff. (New York: David McKay Company, Inc., 1976. Pp. xvii + 334. $9.95, cloth; $4.95, paper.)

It is now fashionable to point out that John F. Kennedy was a cold warrior in foreign affairs and a cautious, pragmatic liberal in domestic matters. So were most liberals at that time. Bruce Miroff has written a political profile of Kennedy and his administration to prove the point that the "progressive" American presidency is inherently a conservative force which must necessarily act to support "corporate capitalism" at home and "global capitalism" abroad. Miroff asserts that revisionist political scientists' writing about the presidency which has sought to reduce idealization of the office is in fact "soft at the core" (p. xiii). This is because the blame for recent tragedies has been directed towards personalities or organization of the presidency. The revisionists still idealize the possibilities of a progressive presidency and assume that the presidential fall from grace is reversible. They want the president to be the source of progressive change in the American system.

Miroff rejects this benign vision. Revisionists among presidential scholars have attacked Johnson and Nixon but have not been sensitive to Kennedy's deep flaws. Miroff thinks this is so because revisionist scholars are "liberals" who are not fully aware of the deficiencies of their own tradition. The purpose of the book is to take the Kennedy administration to task as a typical example of American liberalism in practice. Miroff wishes to show us just how illiberal American liberalism really is.

Miroff is not quite fair to the revisionist scholars. Most, such as Barber, Schlesinger, and Cronin, have been concerned about abuses of presidential character and power. They have accepted liberalism as a context within which presidential leadership must work. Miroff is not really concerned with abuses of character and power, but with the underlying weaknesses of liberal ideology. His general thesis is that the progressive myth of American history is untrue. Liberal presidents do not improve social conditions; the Fair Deal, the New Frontier and the Great Society were essentially fraudulent. In contrast to timidity at home, liberal presidents are bellicose abroad, because the "liberal" task of defending freedom against communism requires the use of American national power in defense of corporate capitalism and reactionary foreign governments. The scope for activism in foreign affairs, however, is much greater than it is in domestic policy.

Kennedy's career is taken as an illustration of this thesis. The seeds of administration foreign policy could be found in liberal political rhetoric in the late 1950s and in the Kennedy 1960 campaign. In 1960 Kennedy picked up the cry of Maxwell Taylor and others about the need for the United States to be able to wage a limited war. This was used as a partisan argument against the Eisenhower-Dulles policy of massive retaliation which sought to reduce defense costs and limit the development of a conventional army. The argument of the need for a limited war capability was later joined by the Kennedy administration with a concern about the Soviet threat to wage wars of national liberation and the result was the American intervention in Vietnam. Miroff documents Kennedy's obsession with crisis and charges that in fact the new president intensified the cold war and misinterpreted Soviet actions by creating a crisis in Berlin out of proportion to the problem and then by treating the Cuban missile conflict as a nuclear crisis, when in fact it might have been handled by political means.

The author finds a shallow liberalism beneath the aspirations of the Alliance for Progress and the American hope that the Diem regime in Vietnam could be transformed into a constructive democracy. This liberal naiveté was in both cases joined to American power, both financial and military. And in time the power overwhelmed the hope.

There are at least three possible explanations for these patterns of belief and behavior. Miroff does not really analyze the possible strengths of each but simply lumps them together in a wholesale condemnation. He seems to think that Kennedy's personality and obsession with crisis explains something. He also implies that popular presidential politics constrains what presidents can do, as in the missile crisis. And then of course there is the question of the combination of idealism and power in American liberalism which, when joined with great energy, can be destructive. However, the author does not really give us the theory of American liberalism which can unite all these things. They are simply jumbled together.

Miroff brings the same kind of indictment to bear in domestic policy. He has kind words for Kennedy and the president's acceptance of the doctrines of Keynes. But then he finds that New Frontier economic policies favored "oligopoly" in that Kennedy and Walter Heller attempted to make a corporate economy work better and failed to cope with the poor and others denied the fruits of that economy. This is criticizing the leopard for his spots because Keynesian economic theory is designed to make a corporate economy work better. However, Miroff simply leaves it there, as if somehow adherence to capitalism crippled American presidents. This may be the case, but there are other constraints on dealing effectively with the problems of poverty and unjust distribution of income. There are constraints in American politics arising from the work ethic and the dominance of a large middle-class population at the polls. There are constraints of knowledge about what to do and how to do it. Finally there is the absence of any viable model of democratic socialism or democratic planning. Even Western European socialist governments find themselves shoring up corporate capitalism in order to meet their own electoral promises. So in a sense what Miroff says is obvious and yet one is at a loss to think of an alternative.

Miroff is on stronger ground in his criticism of Kennedy's hesitancy to leap into the breach on civil rights. Clearly there was a lack of presidential interest in the matter until his hand was forced by the Birmingham disorders of 1963. The author makes clear that the civil rights movement created a tension between the politics of bargaining and control which presidents favor and mass movements which bring a moral intensity to politics which upset the applecart of presidential coalition politics. Lyndon Johnson was to suffer from this tension much more than Kennedy, of course. This is a valid criticism of liberalism, but Miroff does not develop it. Contemporary liberalism has become, in Lowi's phrase, "interest group liberalism" in that diverse organized groups have become much stronger than any mythical public. And the Great Society has become something of a "social porkbarrel." This is due to popular politics, not the influence of corporate capitalism. But Miroff does not see this because, again, he is seeking economic rather than cultural and political causes of the deficiencies of liberalism.

This book is a polemic. It is cast as historical analysis, but the historical narratives are selective rather than definitive. One has the uneasy feeling that the narrative is cast so as to make the author's point without alternative interpretations. I have no quarrel with the specific interpretations of events; one's general feeling about the book will depend upon how one feels about American liberalism. If you agree with the author that liberalism is inherently timid and constrained by capitalist culture and that there is no possibility for an achievement of social justice within the liberal capitalist framework, then you will like the book. If you think it possible to make progress through democratic politics and progressive reform, then you will find the book unsatisfying. Miroff has not quite got it right because he is too intent upon attributing problems to the structure of capitalist society. Progressive presidencies in fact do make advances for broad reaches of the population. But there is always someone left out; that may be inherent in the nature of liberal, pluralist politics.

Miroff never separates John F. Kennedy from the progressive presidency and therefore the book lacks historical perspective. We are never clear whether Kennedy's limitations are those of the presidency or whether in fact there is greater scope for action. Neither Roosevelt nor Johnson is dealt with to any extent and there is no discussion of possible effects of the Vietnamese war and the impact of the Great Society's failures on the liberalism of the future. To some extent Miroff is attacking a straw man. He argues that the belief that a "good" president is sufficient to solve our problems is naive. I agree, but I am not sure that too many people have ever believed that. We know that progressive presidencies depend upon progressive politics; if the nation is not capable of progressive politics, then we will not have progressive, effective presidents. Miroff does not talk enough about the political roots of presidential action, again because he is skittering on the edge of economic determinism.

The book is refreshing in its stubborn refusal to idealize John F. Kennedy. It is time that JFK took his licks along with Johnson and Nixon. The romantic adulation of Kennedy by so many intellectuals is a bit fraudulent; he was in fact more puff and bluster than genuine hero. One might say that my criticism of the book is polemical, simply a projection of my own values and biases. I hope that this is not the case. My primary criticism is that the critique of liberalism is too shallow; it does not explore the cultural and political deficiencies of liberal politics. The author resorts too easily to slogans about shoring up corporate capitalism, which may be an illustration of the weaknesses of American radical thought. Like the liberalism it condemns, it is more bluster and bark than bite because it is essentially a variation of the liberal reform it deplores.

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The Changing American Voter. By Norman Nie, Sidney Verba, and John R. Petrocik. (Cambridge, Mass.: Harvard University Press, 1976. Pp. xx + 399. $15.00, cloth; $5.95, paper.)

Is this book a signal contribution to our understanding of voting? I think so. Is it, as the publishers claim, "the new bible of voting behavior?" I think not. What we have here is a carefully crafted analysis of trends in citizens' attitudes and behavior. Most of the data have been drawn from the surveys conducted by the Survey Research Center and Center for Political Studies at the University of Michigan. The book is an elegant demonstration of what can be done with secondary analysis as well as an abundant record of citizen thought and activity in recent years. As such, it is likely to be frequently consulted as an authoritative reference.

Some of the chapters are particularly illuminating. One of the best is Kristi Andersen's reconstruction of the support brought to the Democratic party by the New Deal where exceedingly fragile data—a retrospective question on whether a respondent ever identified with the other party and a question on the first presidential candidate for whom the respondent voted—have been used with great imagination, and the estimates of party identification perform tolerably well when tested for validity. This reconstruction allows Ms. Andersen to reject a conversion hypothesis in favor of an explanation that potential Democrats were mobilzed by the New Deal, thus producing the Roosevelt majority.

Another very interesting analysis involves a careful tracking of changes in partisanship and policy preference on the part of racial, religious, and geographic groupings. While it could be said that the electorate as a whole was more independent and less centrist in 1972 than in 1952, this statement holds for only one of the eight groupings investigated—high-status northern WASPs. Catholics have become a bit more independent, but a bit more centrist; blacks have become even more Democratic and liberal; white Southerners have become much less Democratic and distinctly more conservative; and so on. This analysis shows that the electorate is indeed a mosaic.

Still another informative analysis was a decomposition of partisan and issue motivations. This yielded estimates of the explained variance that could be attributed uniquely to party, uniquely to attitudes on issues, and shared between both of these voting motivations. It is not news that party is now less important and issues more important, but the extent of the change is striking.

Now if the book does all this—and more—why should I have reservations? First, there is the general theme that *The American Voter* misunderstood the nature of voting because it rested so heavily on a single election. "A primary virtue of measurements extended over a series of elections is that they throw light on the problem of change. So long as the earliest voting studies were confined to cross-sectional relations, they could deal only very inadequately with changes superimposed on those relationships or with changes in the relationships themselves. . . . I have often wondered whether the static . . . determinism of [earlier work] would have emerged from a campaign in which the tides of short-term change were more nearly at flood." This is a sensible thesis, but if the reader has a sense of *déjà vu,* no wonder. The quoted words were written by Donald Stokes in 1965, a full decade before the present authors did their work. I agree with much of what is said in *The Changing American Voter,* but I would have been happier if the phrase "as has been well known for several years. . ." had been frequently used.

A second thesis is that elections should be understood as interactions between the voters and the parties, with voters responding to issues as the issues are structured for them by the parties and candidates. I certainly agree with this argument, but let us have the analysis of party activity that should go with it. We are given exhaustive analyses of the electorate, but on the party side the book has little more than unsupported assertions about what the parties have and have not been doing—for example, on page 356: "There must be some set of political structures to provide issue choices and to convert the votes of citizens into a government. The American parties never did this very effectively."

Then there are some matters of measurement. When I first got the book, I turned at once to the chapter on levels of conceptualization. Alas, the levels of conceptualization analysis had not been replicated. Instead of coding the protocols on the basis of cognitive content, pseudo-categories had been created from the master code. Since this procedure simply identifies the topics mentioned, but not what the respondents have said about the topics, it does not allow inferences about cognitive richness that are essential to reproducing the levels of conceptualization.

The authors also chose to interpret issue voting largely on the basis of precoded questions. Only nine pages—among the most interesting in the book—are devoted to the more complex analysis that is possible with open-ended questions. The correlations between issue evaluations and vote show issues to have been quite important in the 1950s, as did the regression coefficients reported in the 1958 article by Stokes, Campbell, and Miller. Most of the issue voting analysis, though, rests on the precoded questions. Once Nie, Verba, and Petrocik report the open-ended results, they do not return to them.

A substantial portion of the reported increase in issue consistency between 1960 and 1964 can be attributed to the items being analyzed. Nine of the ten pairs of issues concerned either civil rights or the power of the federal government—issues clearly more central to the 1964 than to the 1960 campaign. And a question about the increasingly hot war in Vietnam was substituted for an antiseptic item about having troops stationed overseas. The implication is that there has been a general increase in issue consistency. If, however, one looks at the three non-civil-rights domestic issues that were asked about in both 1960 and 1964, the average correlation (Pearson's r from the *Handbook of Political Attitudes*) drops from .45 to .32, and the average correlation between the three non-Vietnam international issues shifts only from .18 in 1960 to .20 in 1964.

Question format also affects issue consistency. In 1964, the SRC began using double stimulus items (e.g., "In general, some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Others think that the government should let each person get ahead on his own") that imply an underlying scale. What happens if one uses single stimulus items (e.g., "The country needs more jobs and better wages") such as those employed in the 1968 Comparative State Election Project and the 1972 national study directed by Richard Hofstetter at Ohio State? Gamma between the double stimulus integration and welfare items was .49 in 1968; between the single stimulus integration and welfare items it was only .01. The difference is not always this dramatic, and it is not always lower for the single stimulus items. But the average gamma between issue areas was .10 for single stimulus items for 1972. The average gamma reported by Nie for all domestic items (except government size) in 1972 is .49. When the differences are of this magnitude, it just won't do to dismiss them by saying that we all know there is a lot of consistency out there. It may be that much of the issue consistency reported in *The Changing*

American Voter is artifactual.

Now, why not concede the authors the right to choose any indicators they wished? Because in each of these cases, choice of the alternate measure would have led to substantive results opposed to theses maintained in the book. An analysis of levels of conceptualization based on the protocols shows modest cognitive changes. Ideologues increase from 3 percent in 1956 to 6 percent in 1968, and most of the other change is the minimum possible move from group benefits to near ideologue. Use of the open-ended data as the measure of issue voting shows correlations of .63, .58, .60, .64, .66, and .60 in successive elections. Use of other issues, or of single stimulus items, produces much lower levels of issue consistency.

Since rival analyses can be constructed, it would have been nice to have seen a direct confrontation between them. Is it really the case that the war in Korea and the activities of Joseph McCarthy did not stir political passions while the war in Vietnam and the activities of Eugene McCarthy did? Or did the precoded questions used during the 1950s somehow fail to tap the political passions of that time? If this dilemma had been faced squarely, the authors would have had a challenge worthy of their formidable intellectual skills.

Withal, this is a reputation-making book. The senior authors, of course, have no need of this. They are already well established. But *The Changing American Voter* should add luster to the names of Kristi Andersen and John Petrocik who are responsible for some of its most original contributions.

JOHN H. KESSEL

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The Unseeing Eye: The Myth of Television Power in National Elections. By Thomas E. Patterson and Robert D. McClure. (New York: G. P. Putman's Sons, 1976. Pp. 218. $7.95.)

In *The Unseeing Eye* one finds a sound and sensible analysis of the impact of television in the 1972 presidential election. One also finds a book written to be read with enjoyment. Yet the first comment generally heard about *The Unseeing Eye*, among political scientists at any rate, is that the book is pretentious. That charge stems directly from an unfortunate opening page, which says, among other things, that "before this book, no major study had systematically examined television's influence." It goes on, quite unabashedly, to claim that "this book provides what has been missing. It offers the first accurate, detailed information about many aspects of politics that have been hotly debated for the last decade or so" (p. 11).

In fact, *The Unseeing Eye* is an important document and one of the first attempts by political scientists to return to an area that our profession too quickly and too totally abandoned to the sociologists back in the fifties. But *The Unseeing Eye* is not the first attempt to study political television nor is it the only attempt in the field.

As one who has tried hard to get political science into media research, I understand why McClure and Patterson felt the need to exaggerate their seminality. And on balance, I do regard knowledge of *The Unseeing Eye* as a necessary condition for any political scientist who wants to speak knowingly about the content and impact of television journalism in presidential elections. But knowledge of the book is not, as the opening page implies, a sufficient condition.

Happily, McClure and Patterson chose to collect two types of data—content and survey. The content analysis divides itself into two parts. The first part includes "*every* political news report telecast by ABC, NBC and CBS" during the last seven weeks of the 1972 campaign [italics are theirs]; the second part includes "*every* televised political commercial shown by the candidates during the . . . general campaign [italics are again theirs]."

The survey, conducted in a single middle-sized, midwestern town, was an intensive panel study. The overall design produces near nostalgic recollections of the classic studies of Erie County, Ohio and Elmira, New York—studies with which all media students are probably more than familiar. The 731 respondents from the original wave, conducted in September 1972, were contacted at least three separate times during the Nixon-McGovern campaign. A large percentage were reached in the fourth wave as well, immediately following the election.

The two sets of data collected for *The Unseeing Eye* were quite essential in making the kind of case the authors wanted to make. We need more media research that measures both the message and response. Unfortunately this research did not always link the variations in content to variations in behavior as well as one might like. For example, were NBC viewers different from CBS viewers in any way that reflects variations in network content? Still, the data which the authors chose to display—and to display clearly—strongly support the three major and unusually controversial conclusions contained in the book: (1) Network news programs have virtually no influence on the public's view of candidates or issues; (2) Campaign spots on TV have nowhere near enough power to change a voter's mind about a candidate; (3) Political spots on TV, despite their inability to move voters from one candidate to another, convey more information about issues or candidates than do network news programs—*five* times as much, according to this study!

Recurring conspicuously throughout the book are two sub-themes. First is the belief that newspapers are "better" sources of news and have more impact than television news. Second is the notion that the public learns nothing at all from television news and that even popularly held images of candidates are not fostered by television journalism. According to the data, the issues which the networks stress are not the public issues of the moment—because there are no issues on TV news—only hoopla. Television news leads us nowhere politically because, according to McClure and Patterson, television says nothing much at all.

The book reads as an indictment of all three networks far more than of the media consultants who peddle the notion of omnipotent television or of the public who watches. In fact, in an indirect way the public becomes the hero of the book. The voters come away looking very much like rational beings, generally recognizing nonsense or chicanery when they see it.

Unlike most quantitative studies in political science this one not only offers findings, it offers lessons. The last chapter tells the networks they can and should grow up and provide information instead of inane campaign diaries. It also tells candidates that they can and should try "information persuasion" in winning votes—because image persuasion is a bust.

The Unseeing Eye transcends social science and becomes a public service, of sorts. Scholars and lay people have been needing a clearly expressed, data-rich book to counter the outrageous claims, generally made by media consultants, about the magic of television. After reading this book, if there is anything less believable than a TV spot which professes the omnipotence of the candidate, it has to be the spot producer who professes the omnipotence of his or her spots.

But there are some problems with *The Unseeing Eye* that should not go unseen. Even if we ignore the sad problem of a near 50 percent refusal rate in wave one, the study still lacks representativeness. The research is limited not only to one city, it was also limited to seven weeks—the final seven weeks of the campaign. We should be far more willing to accept the representativeness of the respondents in one "representative" town than the representativeness of this limited and extraordinary time period. It is, of course, interesting to learn that in September 1972 those who followed the campaign on television held almost identical images of McGovern as those who followed in print. But by September 1972 we had heard the $1000 plan and we had witnessed the Eagleton fiasco. In all probability, by September, candidate images were already deep in the public psyche. By wave one voters had made up their minds—1,000 percent, so to speak, about Nixon and McGovern.

Now that the 1976 election has passed, it seems especially difficult to accept the premise that television news does not foster images of new candidates. One would have had to have started at least 52 weeks before the balloting to persuade us that Carter's image was not a TV phenomenon in 1976.

There is a second major problem that follows from the total certitude with which so many of the conclusions in this book have been penned. By demonstrating that television news and television spots did not shift images, affect voters or alter information levels very much, one does not prove—as the book might have us believe—that television power is a myth. Politics is much more than electoral choice; my own research suggests that television affects institutions and ethos more than it affects popular vote in the waning days of an election.

Also, in light of Campaign '76, I think we can justifiably argue that television's impact cannot always be validly assessed by focusing on variations in exposure to TV and print. The effect of the Ford-Carter debates may be the same whether one watches, listens or reads them, but that does not deny a TV effect. Immediately following the second debate—the Polish liberation debate—Carter was a meager 4 percent ahead of Ford in public perception as to who won. After one week of TV news Carter was 23 points ahead.

One should accept *The Unseeing Eye* as a serious and successful effort to evaluate the potency (or impotency) of television in a presidential, general election. But one should not forget that politics goes on both before and after elections, and that it may be those 201 weeks of non-campaigning every four years where television news does its thing—and ours.

MICHAEL J. ROBINSON

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Cases in American Politics. Edited by Robert L. Peabody. (New York: Praeger, 1976. Pp. v + 205. $10.00, cloth. $3.95, paper.)

This volume is exactly what it was intended to be—a supplementary reader for an American government course, which includes five excellent case studies dealing with issues of significance in a thoughtful, analytical manner. The essays are well written and even though the headings are traditional (Congress, the Presidency, the Judiciary, Political Parties, Groups) each essay raises questions which transcend easy compartmentalization.

In their essay on Congress, Richard P. Nathan and Susannah E. Calkins tell "The Story of Revenue Sharing" from its historical precedents through the Heller-Pechman Plan to the four sessions of Congress required to enact the State and Local Fiscal Assistance Act of 1972. The essay dramatically traces the growth of interest group activities by the organizations representing hard-pressed mayors and governors until an unusual coalition of Northern Democrats and Republicans succeeded in enacting the measure. Each group saw the proposal differently and, as the essay demonstrates, the more nonincremental the proposal, the easier it was to broaden the base of the coalition. Two procedural problems are handled especially well: the battle for a closed rule (p. 37) and the conference committee compromise over the formula for the distribution of funds (p. 41). The struggle over the rule involved attempts by liberals to tie revenue sharing to tax reform and the desire of the chairman of the Appropriations Committee to have the bill returned to his committee. The closed rule won 8 to 7. Any alternative rule would have led to chaos. The House and Senate formulae had different consequences for each state. The brilliant compromise allowed each state to use the formula that gave it the greatest amount of funding but then reduced each state's total proportionately to stay within the appropriation.

This story is so well told that one only regrets that the authors did not touch on any of the policy questions that were generated by several years' experience with revenue sharing. Perhaps this is the lament which must follow any worthwhile case study.

The presidency essay unravels the complex details of one of the major executive-legislative confrontations of the decade. In "Impoundment of Clean Water Funds: The Limits of Discretion," Louis Fisher traces the legislative history of the Clean Water Act of 1972, the Nixon veto, the override, the impoundment of $9 billion, and the unanimous opinion of the Supreme Court that the administration had acted illegally, noting that, even while losing, the administration had delayed a vital program for 27 months. Throughout American history there have been presidential impoundments, though most of them involved weapons systems and military spending. Fisher notes that the unique feature of Nixon's impoundments was his insistent attempt to create a cannonlike item veto power. In the instant case, Nixon simply asserted that the Congress has been unwilling to fight inflation, thus substituting an executive for a legislative policy judgment. While many members of Congress were hopeful that the courts would solve the impoundment crisis, the judicial insistence on treating each of some 30 cases as a problem of statutory interpretation meant that the Congress had to enact legislation which outlined a precise procedure for retaining control over the appropriations process—thus the Budget Control and Impoundment Control Act of 1974. The Fisher essay notes that the price of abuse of executive discretion is tight language. He might also have noted the expanded use of statutes with legislative or committee veto powers, i.e., laws that prevent certain presidential actions without approval by both Houses, a single chamber, or a specific committee. A *National Journal* survey in 1975 counted 126 such laws, 47 since 1970. Was it only yesterday that the authors of the textbook presidency were warning us of congressional parochialism and glancing enviously at the item veto power enjoyed by so many governors?

Richard E. Johnston's contribution on the judiciary is "Supreme Court Voting Behavior: A Comparison of the Warren and Burger Courts." The essay is an excellent introduction to the workings of the Court and to the historical development of statistical analyses of judicial voting behavior, indicating how different methodologies have frequently produced varying conclusions. In his own analysis of 21 judges over 21 years, Johnston concludes after exhaustive research that, with few exceptions, justices who were Democrats were more sympathetic to the claims of labor and "the economic little man" while justices with prior service as federal prosecutors or federal judges were less likely to vote affirmatively in civil rights cases. As a how-to for presidents seeking to fill vacancies on the Court it is a thought-provoking study, yet the methodology tends to fuzz the complexity of individual cases. For example, if the Burger Court allows an inspector to enter the premises of a company to check for emission of pollutants without a warrant, does that necessarily mean that it is more conservative than the Warren Court, which refused to give health inspectors that right (p. 98)? Furthermore, it is a method that may pay insufficient regard to time as a factor. If the Court deals with issues of criminal law when there is extraordinary public concern with safety in the streets, is it not at least arguable that it might be dealing with the rights of defendants in the same way that an earlier Court might have in the same circumstances?

William J. Crotty's "Anatomy of a Challenge: The Chicago Delegation to the Democratic National Convention" is the essay on parties. It is a chilling chronology of Daley versus the reformers in 1972 with the author finding enough guilt to be distributed in nearly equal amounts to both sides. Mayor Daley's forces may have pretended that the National Committee did not exist, but the high-handed manner by which mathematical quotas were established as a party policy—a point only touched on here—was equally depressing. The victorious struggle to replace the Daley delegation is exceedingly well described, but will do little to encourage students to volunteer their services to a county chairman or to those who celebrate purity.

The essay on groups is "The Energy Issue: Oil and the Emergency Energy Act of 1973–74" by Harmon Zeigler and Joseph S. Olexa. Presumably an analysis of the oil lobby, the essay is also a first-rate case study of the legislative process on a complex issue where dozens of members of Congress were genuinely confused. A bill finally emerged, was vetoed, and the attempt to override failed. Among the issues were an excess profits tax, rationing, a provision to require the oil companies to disclose their reserves, price rollbacks, and, alas, a ban on busing. While the oil lobby is so unique that a case study may mislead students on how the more typical pressure groups function, the significance of oil justifies this study, especially at a time when the industry is advising the nation of its inability to get a fair hearing and when the Congress is still unable to formulate a comprehensive energy policy.

SIDNEY WISE

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Supreme Court Decision Making. By David W. Rohde and Harold J. Spaeth. (San Francisco: W. H. Freeman, 1976. Pp. 229. $11.95, cloth. $5.95, paper.)

Because this book espouses a theory of decision making in the Supreme Court which is the mirror image of the dominant behavioral model of judicial decision making, it should be read together with Glendon Schubert's *The Judicial Mind* and its sequel *The Judicial Mind Revisited.* Like Schubert's psychometric model, the Spaeth approach to decision making (Spaeth had the primary responsibility for the chapters which present the theoretical and methodological bases for the psychological model of decision making) is based upon a psychological model which uses statistical techniques to infer judicial attitudes from court decisions. For this reason the more traditional public law scholars may feel that the model fails to appreciate the uniqueness of courts and judges. This type of general criticism of the behavioral approach is too well known to be recapitulated here.

In a reversal of the Schubert procedure, Spaeth finds the three basic dimensions of judicial values by first constructing a large number of homogeneous cumulative scales and then factor analyzing the resulting scale scores. Thus, the precise meanings of the values "freedom," "equality," and "New Dealism" are permitted to emerge empirically from the data rather than being defined theoretically. Undoubtedly, there is a strong relationship between these three concepts and Schubert's political and economic liberalism scales. In fact, both models produced nine theoretical and five empirical categories of judges because Schubert trichotomized his two major (correlated) attitude scales and Spaeth dichotomized his three (uncorrelated) dimensions.

One innovation touted in the book under review is the separation of cumulative scales into two types—attitude toward objects (AO) and attitude toward the situation (AS)—in order to achieve greater precision. This type of precision, in turn, requires such a large number of cases that a computer is required to keep track of them. This feature of the model attracts press headlines such as this one printed in the *Chicago Sun-Times* of July 25, 1974, "Computer's 8-0 Decision Upheld by High Court." The *Newsweek* (August 12, 1974) caption, "The Court Handicappers," is a more accurate description because the predictions of voting outcomes depend much more upon Spaeth's knowledge of the Supreme Court than upon the cases or scales stored in computer memory banks. Spaeth (p. 148) admits as much when he says the major problem in prediction is judging the "extremeness" (voting split) of a case, which is "normally a matter of judgment rather than a statistical probability."

The Watergate Tapes and Detroit Cross-Busing cases were chosen as "good illustrations of the process of predicting the Court's decisions" (p. 151). Based upon a scale of 17 school desegregation decisions rendered by the Burger Court, the vote in the Detroit case "should have unanimously or nearly unanimously supported cross-district busing" (p. 152). However, contending that "prediction . . . is not so simple," Spaeth decided to use the value "equality," of which school desegregation is a constituent part, rather than the desegregation scale to forecast the outcome of the case. Why then bother to construct specific and homogeneous cumulative scales (AO and AS) if the larger, more comprehensive value dimensions can be used for prediction just as easily? What decision rule can be used to decide the conditions under which a specific scale rather than a value dimension should be used to forecast outcomes? This type of problem overshadows the related problem of coding inconsistencies caused, I believe, by the use of legal rather than psychological categories to classify cases. For example, it is not obvious why restrictive union activities should be AO in one scale (#79) and AS in another (#53). The distinction between AO and AS, though clear in Rokeach's theory, is blurred in application to the Supreme Court.

In the Watergate Tapes case, separation of powers–the chief argument of Nixon's lawyers–was *not* a cumulatively scalable category. Rather than concede that the inadequate data base made the model inapplicable, Spaeth reverted to the legal tradition of precedent to find "two peripherally related decisions that bore on the issue of executive privilege." This plus the knowledge that the Nixon appointees were conservative on criminal procedure gave Spaeth the confidence to correctly forecast the outcome. How is this procedure at all different from the process law professor Rodell went through to predict the outcome of *Baker* v. *Carr?*

The alternative psychometric model presented in this book is interesting in conception but poorly tested in practice. Spaeth's (p. 91) claim to "employ cumulative scaling in a more rigorous fashion than is customary among researchers" is negated by the violation of his own nonrigorous standard for the improvement of the Coefficient of Reproducibility over Minimal Marginal Reproducibility in 34 scales. Moroever, what significance can be attributed to rigorous standards applied to scales composed of as little as two or three cases? (This criticism does not apply to Rohde (p. 192) who regarded 205 cases "unacceptable for scale analysis because they fell into issue areas made up of fewer than ten cases"). These "technical" flaws are extremely important because if "ordinary" standards were applied, 43 percent of the scales upon which the model–and the three dimensions–is predicated would have to be discarded.

In sum, this book is more of a challenge to Schubert's *Judicial Policy-Making* for the undergraduate text market than it is to the psychometric model portrayed in *The Judicial Mind.* The description of the normative and structural context of Supreme Court decision making in the introductory chapters is presented in the clear, crisp, and ordered manner of the lecture notes from which they were taken. Perhaps as an overcompensation for the brief treatment in competing texts, Rohde's extended discussion of opinion assignment does add a dimension of intracourt politics not usually found in most texts on the Supreme Court. The comparative success of modern presidents in appointing justices of similar policy persuasion to the bench, the arguments surrounding the proposed sub-Supreme Court, and the process of forecasting Court decisions are examples of other topics that are likely to engage reader interest.

VICTOR EUGENE FLANGO

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The Polaris System Development: Bureaucratic and Programmatic Success in Government. By Harvey M. Sapolsky. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 261. $9.95.)

The first sentence reads: "Government continually fails us." After briefly illustrating some of the many ways in which governmental agencies typically fail us, Sapolsky writes: "In this book, I am going to describe a government program which worked, a public bureaucracy which was successful. In doing so I will attempt to show what we can reasonably expect from government."

I begin by saying: Books continually fail us. Illustrations are scarcely necessary. But in this review I am going to describe a book which works, a political scientist who was successful. In doing so I will hope to stimulate others to read this book, to learn what we can reasonably expect from scholars.

The first thing that can be said is that this book has already "stood the test of time"–or, at least, more time than is usually required to make a book forgettable. The author's research began in 1967. The first report, which was an earlier version of the book, began to circulate among interested scholars in the fall of 1969.

Although the book appeared in 1972, it remains the finest piece of professional scholarship of its kind that I have ever seen.

What makes this book so exceptional? First, it begins with a clearly stated and important list of questions about an important subject: What are the conditions for "success" in the performance of a public agency? Second, without the usual methodological and theoretical scaffolding that obscures the subject in so many books, this one moves swiftly and surely into the subject. The hard and systematic research was there—including over 400 interviews—but this is one book wherein the researcher and the research procedures do not block a view of the results. Third, the author carefully but unpretentiously draws on a wide variety of relevant substantive and theoretical literature, which in turn should make this book highly interesting to a broad range of political scientists including those who specialize in organizational behavior, interest group behavior, public administration, management, executive-legislative relationships, government-business relationships, government-science relationships, budgetary processes, policy making and decision making processes, bureaucratic politics, and military politics, among others. Fourth, at a time when obituary columns are a light and entertaining diversion from the prose style of most political scientists, Sapolsky writes in an uncommonly literate and readable manner. Fifth and finally, Sapolsky provides an example of a scholar who received research support from an organization closely linked with the organization that he was studying, but without allowing this to bias his findings. Indeed, as an unintended tribute to his integrity, many parts of the Navy covered in this study attempted to block publication of the original report as a book. Never tempted by scandal-type muckraking on the one hand, or raking much under the rug on the other hand, he simply produced a very steady, evenhanded and highly professional piece of work.

Sapolsky, whose modesty is becoming because it is so inappropriate, describes this as "essentially a historical case study." In the middle and late 1960s it was fashionable for distinguished political scientists to dismiss "case studies" as worthless. By the mid-1970s, this view had changed in significant quarters, and prominent individuals such as Bruce Russett (in *Approaches to the Study of Political Science*, ed. M. Haas and H. Kariel, 1972, pp. 428–29) were saying that certain kinds of case studies could be extremely useful. I believe that the Sapolsky book meets the Russett criteria; in any event, it surely meets my own. Readable, interesting, and informative in terms of substance, it unobtrusively sets foundation styles and guideposts for building theory in a number of political science subfields.

Sapolsky indicates here his own determination to produce more case studies within a comparative framework, with the goal of moving toward a general theory of the public management of technology. Indeed, since the completion of this book in the early 1970s, he has been moving along this path, developing new case materials in subject areas as clearly differentiated from this particular book as—for example—medical technologies. I can think of no political scientist better qualified to move ahead into this highly significant but largely unexplored terrain. All best wishes, Harvey Sapolsky!

VINCENT DAVIS

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The Wall of Separation: The Constitutional Politics of Church and State. By Frank J. Sorauf. (Princeton, N.J.: Princeton University Press, 1976. Pp. xiii + 394. $15.00.)

This is a hard book to review because it is so jam-packed with factual material. In looking at 67 church-state separation cases decided between 1951 and 1971, the book "portrays the plaintiffs, attorneys, and the groups bringing the litigation, their strategies and goals, their successes and failures" (p. 5). This is not an exegesis of 20 years of doctrinal development; it is the story of who sues, why, and how.

One of the most interesting of Sorauf's findings is the heavy involvement of three groups in this litigation on the separationist side. They are the American Civil Liberties Union, the American Jewish Congress, and Americans United. One or more of these groups was involved in all but 16 of the universe of 67 cases. The earlier the group was involved in the litigation the greater the likelihood of ultimate success. The influence of counsel for the organizations, e.g., Leo Pfeffer of the AJC, has tended to nationalize this class of litigation. And the strategies adopted by the groups have presented issues to the courts in sharper, fuller form. Sorauf concludes that much more remains to be done before we accept the easy conclusion that groups are not of great importance in constitutional litigation, though he admits that separation of church and state may be a special instance.

The heavy group involvement in this litigation has made it more than usually political. The groups are seeking more than the immediate remedy generally sought by the individual litigant; they are seeking constitutional goals which, of course, are political or policy goals. Another illustration of the politicization of this area of litigation is the fact that 40 to 50 percent of the church-state cases were appealed and, of the 67 cases Sorauf looked at, over half sought access to the U.S. Supreme Court and in 15 percent of the instances got it. And finally, the fact that most of this litigation is begun because of proseparationist failure to win in the legislative arena and that a proseparationist court decision has immediate policy consequences for legislators and administrators, underscores the extent to which church-state cases are deeply enmeshed in the political process.

If the level of group and political involvement is unusual, so are the plaintiffs different from the average citizen. The individual plaintiffs were "predominantly white, male, Protestant, upper middle-class, and well-educated" (p. 145). In this regard they represent those characteristics to be expected of a political elite, but there are certain exceptions. Almost a quarter of the plaintiffs were atheists and nonbelievers or had no religious affiliation. The number of Roman Catholic plaintiffs was predictably low given the general accommodationist approach of that church. Unlike most political elites, almost half of the Protestants belonged to the traditionalist and fundamentalist denominations. The nonbelievers opposed accommodation in any form in principle and the fundamentalist Protestants showed up in cases opposing aid to parochial schools. Particular religious beliefs or the absence of any belief at all was a strong motivating factor for entering into this litigation.

As to occupation, the plaintiffs tended to come from high-status, well-paid walks of life. More importantly, they represented occupations in which they were sheltered from adverse consequences or reprisal for bringing suit—"medical doctors with the Veterans Administration, independent small business men, teachers and college professors, engineers and civil servants in government agencies, and gentlemen farmers, for instance" (p. 147). One might wonder how sheltered some of these occupations are. Sorauf has data on the numbers of plaintiffs who suffered community hostility or worse; it is too bad he did not present a cross-tabulation of that with occupation so that readers could judge for themselves.

Litigation is expensive but only 19 percent of these plaintiffs bore most or all of that expense while 45 percent paid nothing. The latter group, of course, were largely the "token" plaintiffs in suits sponsored from their inception by some group. These plaintiffs were picked usually because they fit the litigating group's view of the ideal plaintiff for this type of litigation.

Sorauf looks at the judges—trial and appellate—as well as at the plaintiffs. The logical expectation is borne out. Jewish judges are the most separationist, Catholics are the most accommodationist, and the Protestants, like their denominations, waver somewhere in the middle. The Protestants tend to be more separationist if the case clearly involves Roman Catholic interest or elementary and secondary schools. The two categories overlap but Sorauf suggests that even when they do, it is education which is the salient issue to these Protestant judges.

Religion does not explain everything about the judges' voting behavior. Method of selection also plays a role, with judges chosen by partisan election being most accommodationist, those selected in a nonpartisan election in the center, and appointed or "Missouri plan" judges the most separationist. The more dependent judges are on the local community, the less likely they are to upset policy accepted by the majority. One interesting related datum: judges with no previous experience were separationist 63 percent of the time while those with prior judicial experience voted that way only 42 percent of the time.

One could go on at much greater length. This is a well-conceived and executed piece of research. It documents carefully in a single area much of what has been written about the judicial process. By confining himself to a single area of litigation, Sorauf avoids the question whether different issues provoke different responses. He also avoids the trap of overgeneralizing from a case study. He is to be heartily commended for convincing his publisher to put the footnotes at the bottom of the page where they belong rather than at the back.

ROBERT H. BIRKBY

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The Alabama Constitutional Commission: A Pragmatic Approach to Constitutional Revision. By William H. Stewart, Jr. (University, Alabama: University of Alabama Press, 1975. Pp. 130. $6.95.)

Like its counterparts in what fortunately is a decreasing number of other states, the Alabama Constitution of 1901 is in need of massive overhaul. Drafted by framers preoccupied with the narrow goal of denying the suffrage to blacks and poor whites and subjected to over 300 amendments, the basic law of Alabama is extremely lengthy, contains much language more suited for the statute books, unduly burdens the state legislature with the concerns of local government, and in general is inadequate to the needs of a modern state. In 1969, the legislature created the Alabama Constitutional Commission to study the state constitution and make recommendations for reform. In 1973, the Commission submitted a draft constitution to the legislature for its consideration. The legislature approved a Commission proposal for annual legislative sessions, but that recommendation failed to win voter referendum approval in the face of intense opposition, principally from major state economic interests. The legislature also failed to act on the Commission's proposal for a code of ethics to govern state officials, but did adopt a comprehensive code on its own. A proposal to reform the structure of the state's court system and improve judicial administration did win both legislative and voter approval, after extensive modification by the legislature. The legislature, however, has yet to consider fully most of the Commission's recommendations.

Professor Stewart's study of the Commission's work should be recommended as supplemental reading in that portion of state politics courses devoted to constitutional reform. The defects in the 1901 Constitution, which Professor Stewart describes, are common to many state constitutions in need of reform. The problems which the Alabama Constitutional Commission has faced—inadequate funding, legislative and gubernatorial indifference, poor publicity, shortage of time, public apathy—are problems reform commissions in other states have confronted or may confront in the future. The recommendations which the Commission made to the Alabama legislature reflect the problems to which state constitutional reformers invariably direct their attention. Finally, although interest groups have generally been indifferent to the Commission's efforts, the annual-sessions referendum did stimulate the sort of interest-group conflict normally expected in debates over state constitutional reform.

Certain of the study's major conclusions are of major significance to those interested in the cause of state constitutional reform. First, if a state constitutional commission is to be successful in its efforts, it must establish a satisfactory relationship with the state legislature. The failure of the Alabama Constitutional Commission to establish such a relationship yet is considered to be a major reason for its relative lack of success. Second, gubernatorial support is also imperative to the success of constitutional reform efforts. Governor Wallace has not yet made constitutional reform a priority item in his administration, and the Commission's problems can also be attributed to that factor. Third, where the legislature meets infrequently, as in Alabama, a special session is necessary for adequate consideration of constitutional reform proposals. The governor's refusal to convene a special session was cited as yet another reason for the difficulties which the Commission's proposals have met. Finally, supporters of constitutional reform must be well-organized if they are to counter those interests committed to the status quo which an existing constitution helps to perpetuate; and a constitutional commission, like the one in Alabama, which for reasons of inadequate support is unable to publicize adequately its activities and recommendations, may be powerless to marshal such support. Along these lines, it might be noted, adoption of the Commission's judicial article was not attributed primarily to the Commission's efforts, but rather to those of the state's legal community, and particularly to the chief justice of the state supreme court.

Apart from its brevity, a principal weakness in this study would appear to be Stewart's failure to analyze in sufficient depth interpersonal relationships and conflicts among the members of the Alabama Constitutional Commission. Group interaction is an important concern of political scientists, and the Commission's membership included a number of provocative figures in Alabama politics, among them John A. Kohn, a sometime legal and political advisor to Governor Wallace and reputedly the author of Wallace's bizarre 1963 inaugural address. A more thorough examination of group interactions on the Commission might have improved the study's quality. The study might have been enhanced by in-depth interviews with key figures in the reform movement to augment the newspaper articles and Commission reports which apparently form the core of Stewart's research material. Despite these weaknesses, however, the study is a useful addition to the literature on state constitutional reform.

TINSLEY E. YARBROUGH

East Carolina University

Rights of Union Members and the Government. By Philip Taft. (Westport, Conn.: Greenwood Press, 1975. Pp. 348. $14.95.)

Philip Taft, a distinguished senior labor historian, has written a work which, although important, is not theoretically *au courant*. At a time when social scientists are trying to appraise internal union politics in terms of categories of analysis more sophisticated than those provided by Lipset and Trow's *Union Democracy*, Taft offers no new sociological assistance. At a time when policy analysts are trying to focus on union prospects in terms of contract negotiations under conditions of a stagnant economy, Taft presents no new relevant economic postulates. Nor does the book seem in touch with the full range of current events affecting organized labor: the lament of civil rights workers over shortcomings in the "Philadelphia Plan," the distress of labor scholars over flagging efforts on behalf of civil rights training in labor study groups, the concern of civil libertarians about corporate wiretapping and management spying. Taft offers no discussion useful to policy makers preoccupied with the politics of stalemate affecting the regulation of migrant workers. In short, the book does not fully deliver on the title's promise. Yet it is an important volume. Its importance can be appreciated best when one realizes that, the broad title notwithstanding, the author neither promises innovative social science analysis nor seeks to present a review of the many problems of current events that immediately rush to mind under the heading of the rights of union members.

Taft's book successfully develops a narrowly defined focus. The volume is the most comprehensive available analysis of the Labor Management Reporting and Disclosure Act (LMRDA)–the Landrum-Griffin Act of 1959. The statute regulates various internal union affairs, including the control of funds. It forbids union office-holding by former convicts and Communist party members. (On the latter point, the author's analysis of *United States* v. *Brown*, 381 U.S. 437, is wholly inadequate.) The Act also guarantees free speech and a variety of other rights for union members as well as periodic secret elections. It supplies, in Taft's nomenclature, "a bill of rights for union members."

Taft has set for himself the task of meticulously reviewing and tabulating the working effects of the LMRDA as those effects are shown in rulings on grievances brought to trial by union members, union leadership, industrial management, and departments of government. Taft's work is impressive in panoramically reviewing published cases from both state and federal courts. For example, the book elaborately discusses the reporting and disclosure provisions of Title II, designed among other things to prohibit embezzlement of union funds, and analyzes over 800 criminal actions between 1960 and 1972. Taft tabulates indictments found, lists amounts embezzled, and assesses how the statute functions from the points of view of union administration and industrial management. Taft also discusses the Secretary of Labor's role in enforcing fair internal elections in the light of the congressional hope that democratic elections would be the most effective antidote to the evils of bossism, and that the sordid business of misuse of funds could be checked by overcoming the membership's ignorance through full financial reporting. Taft is clearly in sympathy with these aspirations.

In recent years, Chief Justice Burger has called upon Congress to sponsor litigation-impact studies of proposed new legislation; Taft's book illustrates the broad scope that such policy analysis might undertake. Increasingly, social science students of public law are calling for research on multiple decision-making and dispute-settling forums (not just the Supreme Court). In this light, Taft's book has explored new scholarly terrain. Nevertheless, the reader might well wonder whether the author would accept these laurels, for the book reflects scant awareness that he is intent upon anything but a monograph in history wholly innocent of closely related contemporary social science.

RICHARD P. CLAUDE

University of Maryland

The Compromising of the Constitution: Early Departures. By Rexford G. Tugwell. (Notre Dame, Ind.: University of Notre Dame Press, 1976. Pp. xi + 188. $8.95.)

American constitutional historians generally agree that it was the national government's incapacity to govern during the Confederation which led to the Constitution of 1787. The need to remedy that deficiency held the delegates at Philadelphia together, but beyond that they disagreed sharply about the central government's structural arrangements and the nature of its relationship to the states. For 112 days they debated their differences, resolving many, and consigning others to future generations. In the end they produced an ingenious document which reflected the arts of conciliation and compromise. Today, almost 200 years later, their handiwork, with its 26 amendments, survives despite social, economic and political changes that the framers could not have foreseen. But has it survived intact? Have the adaptations since 1787 to what Chief Justice Marshall called the "crises of human affairs" compromised both the Constitution and the legitimacy of the governmental system which it created? These are the profound questions which Rexford Tugwell attempts to address in this book.

Tugwell has focused his research on the Constitution's contemporary relevance ever since his distinguished careers in teaching and government brought him to the Center for the Study of Democratic Institutions at Santa Barbara, in 1964. His professional background, which now spans more than 50 years, includes service as Governor-General of Puerto Rico, Chief Planner for New York City, five years as one of FDR's original brain-trusters, and teaching positions in economics and political science. Those experiences equip him to apply both theoretical and practical perspectives to the study of constitutional government. His contemplations at the Center have convinced him that the Constitution has now become an obsolete and illegitimate charter which needs a comprehensive overhaul.

In this book Tugwell spares us another draft of his ideal constitution for modern America (his fortieth draft appeared in *The Emerging Constitution*, 1974). Instead, he tries to explain in historical terms how a "quagmire" of interpretations and inferences which began in 1789, and which he calls "early departures," have distorted the Constitution into a "network of illegitimacy." He asserts, usually without adequate documentation or logical basis, that the framers made formal amendment of the Constitution almost impossible in Article V even though "none could have expected or wanted it to endure intact for very long" (p. 4). But changing circumstances, and the Constitution's ambiguities and omissions made revisions necessary from the outset in 1789. Tugwell maintains, quite unconvincingly, that the obstructions to change embodied in the formal amendment process caused the nation's early leaders to resort to "amendments by implication," which they achieved through a variety of interbranch bargains and assertions of judicial supremacy. Those "early departures" from the Constitution's "directives" included their decisions on the creation of executive departments, the dismissal of executive officials, executive privilege, judicial review, impeachment grounds, Congress' role in foreign policy and its power to investigate. Tugwell concludes that since *the people*, from whom constitutional authority emanates, have had no opportunity to legitimate the "amendments by implication," the Constitution itself has become compromised, and its governance of the nation has become more mythical than real.

Those who are even moderately familiar with American constitutional theory will find many flaws in this book, aside from its awkward syntax and abundance of non sequiturs. The most serious, because they are so crucial to his analysis, are the author's misconceptions about the Necessary and Proper Clause and the Tenth Amendment, which he regards as irreconcilable contradictions. He actually says that "the Tenth Amendment restricts national powers to those *specifically* delegated to the United States or prohibited to the states" (p. 102, italics mind). That calls to mind Justice Rufus Day's same blunder in *Hammer v. Dagenhart*. Tugwell's miscomprehension leads him to conclude that the Tenth Amendment, notwithstanding the Necessary and Proper Clause, restricts the national government's implied powers. In his words, after the Amendment's adoption, "There would be no expansion by implication. That is what the Tenth Amendment *plainly* says" (p. 103, italics mine). But it does not say that, plainly or implicitly, and Madison, its reluctant architect, never evinced any such contemplation. To the contrary, Madison proposed it only to quiet antifederalist fears that an overbearing central government might destroy the states, and he successfully resisted efforts to use the words "expressly delegated." Madison's belief that the amendment added nothing new to the Constitution has never been discredited. Tugwell's novel idea that it was designed to constrain the national government's implied powers is historically untenable.

He also founders in his attempt to explain just why the nation's leaders came to rely on "interbranch bargains" and constitutional interpretations in developing national governmental powers. Tugwell concludes that their failure to use the formal amendment process, or to make it more practicable, reflected a "general willingness to overlook departures [from the Constitution], however gross. This could only happen when there was the pretense that what was being done was not happening. Attenuations . . . were said to be merely interpretations" (p. 163). This argument ascribes too much to a new kind of conspiracy theory and fails to account for the proposition that successful operation of the American constitutional system requires a workable combination of separateness with interdependence, and autonomy with reciprocity.

In all, this is a most disappointing book. The author is at his best when he discusses the merits of planned constitutional development, but at his worst when he strays into constitutional theory and history. Tugwell has an unremitting devotion to constitutional government and impeccable credentials as a scholar. Let us hold him in high esteem for the works that brought him the Woodrow Wilson and Bancroft Awards in 1957 and 1968, and let us look forward to a continuation of his prolific scholarship. But let us refrain from gauging his metal on the contents of this book. Rexford Tugwell, now in his mid-eighties, deserves better.

JAMES L. ELSTON

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Continuity and Change: From the Warren Court to the Burger Court. By Stephen L. Wasby. (Pacific Palisades, Calif.: Goodyear Publishing Co., 1976. Pp. xii + 236. $6.95, paper.)

During the presidential campaign of 1968, Richard Nixon pledged that if elected he would appoint individuals to the Supreme Court who would not read their own social and political ideas into their decisions. Within three years of his election, President Nixon had appointed four men to the Supreme Court including the new Chief Justice, Warren Earl Burger. *Continuity and Change: From the Warren Court to the Burger Court* is an analysis of the impact of the Nixon appointees upon Supreme Court policy making.

Beginning with an examination of the values and attitudes of the Nixon appointees (Burger, Blackmun, Powell, and Rehnquist), Stephen Wasby emphasizes that President Nixon, although not without embarrassment (the rejection of Haynsworth and Carswell), succeeded in placing four moderate to conservative justices on the Court because many in the Senate shared Nixon's views or were prepared to accede to increasing public opposition to Warren Court policies. The Nixon appointees were to redirect the Court's policy-making course and bring it into line with the climate of opinion prevailing in the late 60s to early 70s. However, the Nixon appointees do not constitute a majority and this fact, as Wasby demonstrates, has had significant effects upon the policies of the new Court. In an excellent chapter (chapter 3, "Internal Dynamics"), Wasby points out that although the Nixon appointees are the "dominant force" on the Court, they cannot control the Court's decisions without the help of two Warren court carry-overs, Stewart and White. Moreover, the Nixon appointees themselves, although a cohesive voting bloc, have not displayed monolithic unity. Thus, even if by the 1972 term "the classic Burger Court voting alignment is a 6–3 ruling" (with Douglas, Brennan, and Marshall—the core of the later Warren Court's liberal majority—in dissent), a surprisingly high degree of fluidity and unpredictability is still evident on the Court.

Wasby's analysis of the internal dynamics of the Burger Court is vital to an understanding of the Court's policy outputs. Wasby devotes five of the book's nine chapters to an examination of the Burger Court's decisions through the 1973 term (an Epilogue surveys the 1974 term). It is clear that the fears voiced by many liberals regarding the rollback of Warren Court policies have not been completely justified, for the Burger Court has not been consistently conservative in its decisions. As Wasby points out: "In looking at the transition from the end of the Warren Court to the first years of the Burger Court, we have seen a mixed picture instead of clearly defined results" (p. 206).

This mixed picture is particularly noteworthy in two areas which produced the most opposition to the Warren Court, school desegregation and criminal procedure. In the former area, the Nixon "Southern Strategy" was dealt a blow in Chief Justice Burger's first term (1969) when the Court rejected an administration request for delay in southern desegregation and ended the era of "all deliberate speed." Moreover, the Burger Court has validated the use of bussing and broadened the grounds for attacking de facto segregation in the North. At the same time, however, the Court refused to extend its decisions any further, for while upholding the validity of bussing within northern cities, it has refused to sanction urban-suburban bussing plans. In the area of criminal procedure, the Burger Court has been less prodefendant than its predecessor as can be seen most clearly by the serious inroads which have been made into Warren Court restrictions on searches and seizures and the use of confessions. However, *Mapp* and *Miranda* have not been overruled, and the Burger Court has not been uniformly prolaw enforcement as evidenced by the invalidation of warrantless domestic security electronic surveillances; the extension of the right to counsel to any criminal charge where imprisonment may result; and the granting (albeit cautiously) of procedural protections to probationers, parolees, and prisoners. Thus, while the Burger Court does place greater emphasis on "law-and-order" (in an ironic twist, this can also be seen in the *Nixon Tapes Case*), results have not been unmixed.

Wasby indicates that in those areas where Warren Court policy was unambiguous (e.g., the firm prohibition of aid to parochial schools), the Burger Court has generally continued the policy. On the other hand, in areas where Warren Court policy was not clear or had provoked serious intracourt dispute (e.g., obscenity, the standards to be applied in reapportionment cases), the Burger Court has not hesitated to formulate its own generally more conservative rules. Similarly, in responding to questions not considered by the Warren Court, while the Burger Court has basically acted more conservatively than its precedessor would have (e.g., newsmen's privilege, property tax financing of education), there have been some liberal surprises (most notably abortion).

Wasby's analysis makes it clear that the Burger Court has not brought about sharp cutbacks in Warren Court policy. Although there has been serious erosion of doctrine in a few areas, the Burger Court's conservatism has been expressed more in its refusal to advance Warren Court policies, in its general unwillingness to formulate broad doctrine in favor of a case-by-case approach, and in its refusal to hear certain kinds of cases, than in the repealing of Warren Court policy. As Wasby concludes: "We thus find substantial doctrinal continuity between the two Courts, their relation being characterized generally by an absence of the next step forward rather than by any sharp break or discontinuity" (p. 207).

Although the Burger Court has not been uniformly conservative, the Nixon appointees have succeeded in lowering considerably the Court's profile. The Burger Court is generally more restrained, more deferential to the actions of governmental officials, and places less emphasis on the rights of the individual than its predecessor. Acknowledging that this lowered profile has brought the Court closer to the political realities of the day, Wasby concludes by questioning whether this should be the Court's overriding concern for, given the many political and bureaucratic obstacles, the justices' rulings are seldom fully implemented in any event. "Therefore one might demand that the Court lean harder in the direction of emphasizing our nation's constitutional ideals, model itself on the Warren Court, and thus serve as a major source of our aspirations and ideals" (p. 210).

Wasby has done a most commendable study of the Burger Court. The book is well written, findings and conclusions are presented clearly and are fully documented. It is a much-needed contribution to our understanding of the changes now taking place on the Supreme Court.

HARVEY R. KORNBERG

Rider College

One Environment Under Law: A Public-Policy Dilemma. By Lettie McSpadden Wenner. (Pacific Palisades, Calif.: Goodyear Publishing Company, 1976. Pp. xiii + 170. $5.95, paper.)

In the mid- and late-1960s, a major development in the United States was a substantially increased concern with environmental quality. A growing number of Americans finally realized that continued exploitation of the nation's air, land, and water could only lead to an ecological catastrophe. Thus, to cope with existing environmental problems, many of these new environmentalists demanded a plethora of new environmental laws in the belief that virtually any change in the government's activity would improve the situation.

With the benefit of several years' hindsight, one can now say that these statutory changes have had some beneficial effect. Many sources of pollution have been controlled, and both air and water are generally cleaner now than in the 1960s. Despite these improvements, however, serious problems still remain. Moreover, recent ecological disasters have served to intensify one's awareness of both the complexity and interrelatedness of environmental problems.

As Wenner's analysis suggests, this awareness has also emphasized the fact that the remedies that placated many people in the past are likely to have only a negligible impact if the nation wishes to avoid substantially more serious decay and degradation in the future. This situation exists because most current environmental-control programs are directed at proximate or immediate causes of pollution (e.g., an economic system that encourages environmental damage or a political system that discourages administrative coordination). In contrast, the more important societal causes of environmental decay (e.g., growthmanship, industrialization, and population growth) receive only scant attention from public policy makers. Such issues as the need to control population size or to modify a society's expectations of continual economic growth are clearly controversial and unpopular political stances. Yet, policy makers' apparent rejection of these causes of environmental degradation affects the choice of solutions. Although proximate causes of pollution can often be remedied by incremental changes, societal causes may require drastic and nonincremental policy responses. Wenner, however, is decidedly pessimistic about the likelihood of such drastic changes, especially in view of current practices. In an especially well-integrated discussion of present pollution controls, Wenner demonstrates that environmental policy making is typically incremental. It tends to be fragmented, short-sighted, noncomprehensive, and highly reliant on previous decisions.

In addition to the barriers to change created by incrementalism, Wenner also explores a substantial number of frustrations that environmentalists face when they attempt to bring about major changes. For example, Wenner, unlike some other environmentalists, recognizes that courts have traditionally favored polluters and that, even today, environmentalists should remain skeptical of a strategy that relies heavily on litigation. Since most polluters perform important functions for society, Wenner says "there is little real reliance on the criminal sanction in the case of a company or municipality discharging untreated wastes. The discharger is merely informed of the illegality of his actions and asked to modify them" (p. 142). This "soft-on-polluters" attitude is a major reason why "persons affected adversely by pollution demanded that legislatures create the administrative framework that exists today" (p. 141). Unfortunately, the environmental administration that exists as a result of this dissatisfaction is often beset by uncertainty, ambivalence, and a lack of political support. Moreover, administrators intent on abating industrial pollution frequently find themselves in glass houses since state and local governments are among the worst polluters. Such situations obviously create the public-policy dilemmas of which Wenner writes.

One Environment Under Law is intentionally concise, but the author is still able to provide a good picture of most major environmental issues. There are, however, some important areas that are either absent or inadequately developed. The absent areas include the role of politics in defining acceptable levels of pollution and the controversy over the availability and effectiveness of control technology for such stationary sources of air pollution as power plants. The latter is a surprising omission in view of the increased emphasis on the use of coal, which can be a major source of pollution when burned. Similarly, at least some sections are inadequately developed. For example, the author notes that "auto-emission controls constituted the first and only congressional mandate to EPA to undertake directly enforcement of pollution controls without the assistance of state government" (p. 90). Wenner asserts that state and local governments were excluded from responsibility for automobile pollution because they were too responsive to private industrial demands. An equally and, perhaps, more appropriate, explanation is ignored—namely, that automobile manufacturers actively sought exclusive federal responsibility in order to avoid conflicting regulations by each of the 50 states.

Despite these reservations, Professor Wenner has produced a well written and provoking analysis. In fact, she has provided a useful addition to the literature on environmental politics.

RICHARD J. TOBIN

State University of New York, Buffalo

Comparative Politics

Co-Operative Farmer and Welfare State: Economic Change in an Israeli Moshav. By Jay S. Abarbanel. (Atlantic Highlands, N.Y.: Humanities Press, 1974. Pp. 236. $13.50.)

This is a study of ideological change in response to economic realities, hardly a new theme. Still, since the extremely intriguing world of Israeli politics has not been particularly well-served by scholars, the Israeli variations on this rather ubiquitous theme are well worth studying. Furthermore, while most earlier studies of political change in the Israeli moshav (a collective village where farmers own equal plots of land, less well known but more widespread than the kibbutz, where no private ownership is allowed) have dealt with the adaptation of new immigrants (mostly from Afro-Asian countries) to a new cultural milieu as well as a different value system, this study deals with veteran Israelis and immigrants from Western countries who are quite familiar with Israeli society and its values.

The basic thesis put forth by Abarbanel is that rapid economic change dangerously undermines the basic values of the moshav (although admittedly, members of Sharon, the particular moshav he chose to study had lacked strong ideological commitments to its particular way of life from the very beginning). Abarbanel argues that "the process of social and economic change that is at work among the farmers of Sharon towards greater emphasis on individual as opposed to collective achievement is not only a local phenomenon among a group of farmers in a small settlement, but is also a more general development within Israeli society" (p. 224). In this process, "collectivism and co-operation become techniques rather than values themselves" (p. 223). Due to rapid economic change, the resulting social differentiation and political tension, the farmers "... recognise the predicament between individual equality and individual achievement; and it is the shift towards individual achievement that concerns most men" (p. 227).

In pursuing his major theme, Abarbanel describes and analyzes in painstaking (and at times, overwhelming) detail the salient features, of the economic, social and political system of Sharon. This is a micro, anthropological approach, inspired by Max Gluckman, whose other disciples (E. Max, S. Deshen, A. Cohen and M. Arenof, among others) have made outstanding contributions to the study of politics in various Israeli communities. The author spared no effort in documenting processes of different rates of economic development and trends toward more individualistic behavior, which according to his findings are determined by the phase of family development, level of education, ethnicity and outside economic resources. Political scientists will be particularly interested in the analysis of the local interest groups, which cut across economic and other boundaries.

The major weaknesses of the book are twofold. First, it fails completely to integrate its theme into the existing huge literature on ideological change, which the author by and large ignores. Admittedly, this is an anthropological study of economic change on the micro level; nevertheless, since eventually it turns out that the theme is that of values and ideological response to economic change, the author could have been expected to refer to the abundant relevant theoretical findings along these lines. The second deficiency is the weakness of the links between the macro levels. Although Abarbanel claims that the process he describes is "also a more general development within Israeli society" (p. 224) he fails to prove or even develop and demonstrate this extremely important statement. Again, he fails to refer to the relevant literature on Israeli politics (and in particular ideological change in Israel), thus missing the opportunity to link his particular case to the larger universe of the post-collectivist phase of Israeli politics. The broader political implications of the study, of great potential interest to political scientists, are not seriously explored. Similarly, the chapters describing general processes and features of Israeli society and politics seems the weakest of the book.

Because of these shortcomings, Abarbanel's main contribution remains that of describing and analyzing a single, fairly limited case. On that level, his scholarship seems impeccable and impressive indeed.

GABRIEL BEN-DOR

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French Legislators 1800–1834: A Study in Quantitative History. By Thomas D. Beck. (Berkeley: University of California Press, 1974. Pp. 216. $13.75.)

Thomas D. Beck marches with that brassy band of historians, sometimes called "cliometricians," who stride resolutely into forests of forgotten figures searching for a more rigorously scientific understanding of humanity's past. Their means are respectable and their goal laudable, but their results are not always commensurate with their effort. This book illustrates both the merits and the pitfalls of the genre.

Beck casts his light on a period and a country that have received little such attention. He poses interesting and important problems: "What was the meaning of the return of the Bourbons, what were the consequences of the Revolution of 1830, and ... what was the true social reality of the period?" He has gathered a mountain of statistical data—presented in 40 pages of appendices—and analyzes it in the text.

His organization is spare and orderly. His first substantive chapter examines correlations between selected social, economic, and demographic (SED) characteristics of *départements* and the partisan orientation of the deputies they elected. He calls it "Modernization and Political Opinion." The other six chapters cover the parliaments in six successive periods between 1800 and 1834. They describe briefly the political and constitutional context of the parliaments, their general orientation, the electoral systems, and a few individual members. However, their main concern is the character of the deputies in terms of social status, occupations, age, wealth, and political experience. Also, Beck examines the correlation of regions with partisan orientation.

Beck's research illuminates pitilessly the special hazards of cliometrics. He cannot analyze legislative roll calls because his parliaments voted secretly. He finds partisanship a treacherous variable because formal parties had not been organized yet. He cannot direct indices of modernization in statistical form so he makes do with other SED data, some of which were nonexistent before 1847. He lacks even those data for constituencies and has to fall back to the *département* level, with consequent loss of precision.

Beck's persistence in the face of such formidable obstacles is laudable and his success impressive. So is his candor in acknowledging the limitations of his data. However, some of his analytic techniques are not entirely clear to me. For instance, I am puzzled by his failure to present some SED data in per capita terms, thereby neutralizing the large variations in size of population and area among *départements*. Also, his delineation of regions has no apparent historical or logical basis. Again, his "left-center-right" labels for nonideological groupings of deputies seems anomalous. Finally, his reliance on correlation analysis without examining the explanatory value of his variables is not very helpful in providing an understanding of their real significance.

Beyond these technical matters, it seems to me, the force of Beck's study is weakened by the absence of a systematic thesis. He may have been hobbled by the lack in his data of a clear pattern of sufficient significance and interest to support a thesis. Also, his findings may lack sufficient novelty to bear a thesis because they tend to confirm existing interpretations. In his concluding chapter he does take a parting shot at David H. Pinkney's *The French Revolution of 1830*, but my comparison of their two interpretations suggests more quibble than quarrel. Nevertheless, a central argument—however modest—would have helped the reader grope through the swirl of statistics.

Despite these shortcomings, Beck's contribution should not be overlooked. In the first place, his book contains a great deal of useful information not readily available elsewhere and a clear and straightforward account of parliamentary electoral history for the period. Secondly, as he says, it has "clarified many things we already knew." Third, it provides a valuable test of existing interpretations. Valid statistics used properly have no equal for precision, concreteness, and accuracy in communications. Traditional historians should be subjected to such testing. Unfortunately for the testers, they cannot know in advance where error lies and confirming studies are intrinsically less interesting and significant. This should not lead us to discount their value. After all, they also serve who only count and weigh.

WILLIAM G. ANDREWS

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On Revolt: Strategies of National Liberation. By J. Bowyer Bell. (Cambridge, Mass.: Harvard University Press, 1976. Pp. xv + 272. $15.00.)

Ambivalence is the predominant feeling one experiences as one reads *On Revolt.* On the other hand, one marvels at the wealth of material; on the other, one is dismayed that this material is not guided or framed by any explicit consideration of theory.

Bell's articulated objective is to examine a particular set of "revolts"—those that erupted against the British Empire in the postwar period—in search of patterns and uniformities. More specifically: "I was most concerned with the slow unfolding of revolutionary strategy, the calculated response to repression, and the shift of tactics under pressure—and much, if not all, of that remained in individual memories" (p. vii).

Accordingly, Bell focuses upon a series of case studies including the Irgun, the Gold Coast, the Malay insurgency, the Mau Mau, the Egyptian Fedayeen, the EOKA in Cyprus, the NLF in South Arabia, the IRA. In addition to closely examining primary and secondary sources, Bell interviewed dozens of diplomats, bureaucrats, arms smugglers, missionaries, mercenaries, journalists, and rebels in several countries.

In the opening chapter Bell makes a feeble attempt to theorize by distinguishing between revolution and revolt. Revolution "at least implies substantial change, a most serious transformation of an existing society beyond the arrival of a new elite. . . . A revolution seeks to transform or transpose basic values" (p. 4). By contrast, "a revolt is a coherent, armed rising of sufficient proportion to challenge seriously the existing central authority, but without the capacity to create an alternative authority: a lethal dialogue between rebel aspirants to power and the forces of authority" (pp. 7–8). Since rebels characteristically launch their armed struggle in the name of national liberation, Bell proceeds to distinguish two "models" of national liberation movements: "ideological" and "nonideological"—a distinction that is neither clear, nor convincing, nor necessary, nor useful.

Having discussed "the British imperial stage" around WW II, having emphasized the British policy of "devolution"—"ceding power from the center to those advanced and developed imperial areas grown restive under centralized control" (p. 20)—Bell plunges into his case studies. Here the range and the depth of his knowledge is undeniably impressive.

Bell's concluding chapters emphasize the policy of devolution and the British notion of legitimacy as the latter is granted to or withheld from various movements and groups. He declares: "Those gunmen and terrorists who sought power outside the Commonwealth route were without legitimacy" (p. 168). Thus, for example, the Malayan and Mau Mau movements were crushed whereas the Irgun movement was not. In either case, the confrontation between the incumbents and the rebels involves some form of a "lethal dialogue" (see above)—an expression which, although Bell insists upon it, does not enlighten us.

The pivotal element in revolt, Bell finds, is the existence of a small number of dedicated individuals. "A revolt does not begin because of optimum conditions. What makes it possible is not the chance of success or failure but the simple commitment by individual men to victory or defeat" (p. 179). Beyond this, Bell discovers the importance of political and military organization, an effective ideology to mobilize the masses, and the implementation of such revolutionary techniques as guerrilla warfare and terrorist activity. In this context, he cautions against blind commitment to abstract theory: "Though massive, revolutionary theory is often only tangential to practice. . . . Pragmatism is everything. . ." (p. 236).

On Revolt is successful by conventional criteria, in that the author delivers what he promises. The book's major shortcoming, on the other hand, is that it is not informed by an explicit and coherent theory. In fact, judging by Bell's discussion and his sources, he is not sufficiently familiar with the theoretical literature on revolution and national liberation. Nowhere does the central concept of "strategy" (note the book's subtitle) receive anything approaching sustained treatment, though it is loosely used with great abandon throughout the volume. Moreover, contrary to the opening distinctions, revolt, revolution, rebellion, and national liberation are used interchangeably throughout. True, one could probably construct a theory of national liberation movements based upon the materials presented in *On Revolt,* but that is quite another matter.

MOSTAFA REJAI

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Chinese Socialism to 1907. By Martin Bernal. (Ithaca, N.Y. and London: Cornell University Press, 1976. Pp. 259. $15.00.)

The intellectual cataclysm that overtook China around the turn of the century is only beginning to be studied. Within a single decade, 1895–1905, the Confucian political tradition was discredited and a stream of alien ideas—science, social Darwinism, English liberalism, socialism, anarchism—flooded in from abroad.

In this first volume of a projected trilogy on Chinese social movements to 1915, Martin Bernal examines one current in the stream. Knowledgeable about Western socialism, he shows how it entered China first through Protestant missionary publications, notably the *Review of the Times* (on which he has a whole chapter), and after 1900 through Chinese students in Japan. His chapter on Japanese socialism is a mini-monograph in itself and stresses the unsung career of Kōtoku Shusui whose intellectual trajectory from Marxism to anarchism was noted by Chinese revolutionists. He assesses the influence of Western socialist writings on major Chinese radicals, Kang You-wei, Sun Yat-sen, Liang Qi-chao, and writers for *The People's Journal* such as Hu Han-min and Zhu Zhi-xin.

Bernal acknowledges that socialism before 1907 was a secondary theme in Chinese thought. We should know about it, he believes, because it has been ignored. Scholars "have begun their studies of Chinese communism with 1919" (p. 6), and even Mao has written that, before the October Revolution, Chinese "did not know of Marx and Engels." Bernal observes that the study of early socialism can encourage historical questions. Why was pre-Leninist socialism not relevant to China before 1919? What influence had it on later Chinese communism? Did Chinese tradition impose selective modifications on Western theories, and did these modifications "form a coherent pattern" (p. 7)? Although these and other questions constitute a good analytical framework, Bernal does not use it as fully as he might. He pursues "socialism" so attentively that he does not always relate it to the larger revolutionary process.

Can we, for example, discern in early Chinese socialism a "pattern" that we could apply to the study of Chinese liberalism or Chinese constitutionalism? Was there perhaps a "sinicization" of socialism that would give us new perspectives on Mao's later "sinicization of Marxism?" At various points in the book Bernal shows how particular concepts, such as *datong* (Great Harmony), the *jingtian* (Well-field) system, or the *juntian* (Equal-field) system, influenced Chinese perceptions of Western social thought, but he does not draw out a general pattern.

One approach might be to consider the Chinese resistance to class analysis, which Bernal records incidentally although he does not interpret it. Sun Yat-sen "saw his land policy as a prophylactic, a method of avoiding social division" (p. 60). Liang Qi-chao, who denied China had classes, favored "slow progress to state socialism on the German model" (p. 104). Zhu Zhi-xin translated the *Communist Manifesto* so as to mute its call for class war (p. 117). And when socialism gave way to anarchism during 1907 it freed radicals from "worry about such aspects of social democracy as class conflict" (p. 224). Today, when the slogan "never forget class struggle" is heard throughout China, we should wonder why important radicals did not accept a class analysis early in the century. Is their readiness to do so after 1919 to be explained by sociopolitical changes within China, or by the new Leninist content of the socialist message after the Russian Revolution?

Within his narrower focus, however, Bernal provides a valuable record of China's early reaction to socialism. He gives thoughtful answers to such questions as, How did Sun Yat-sen evolve his conception of land nationalization? How can we account for the different approaches to socialism by Sun Yat-sen and Liang Qi-chao? What is the relationship between political and social revolution in the writings of Zhu Zhi-xin? His article-by-article treatment of the journalistic controversy on social policies between Liang Qi-chao and the Revolutionary Alliance (1905–1907) captures more fully than ever before in English the struggle of the expatriate radicals to define the significance of Western socialism for China. His last chapter, perhaps the best, analyzes the rise of anarchism in relation to the new cultural conservatism (Liu Shi-pei, Zhang Bing-lin) that crept into the revolutionary movement after 1905. The complex relationship between socialism and anarchism, each highly eclectic, will be a challenging subject for Bernal's future volumes.

Finally, the Cornell University Press should be commended for using the *pinyin* system to transcribe Chinese names and terms. A standard romanization style is in sight at last!

CHARLTON M. LEWIS

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The Politics of Change in a Zambian Community. By George C. Bond. (Chicago and London: University of Chicago Press, 1976. Pp. vii + 178. $13.50.)

Political change occurs as individuals mobilize their resources and structure social reality *in pursuit of personal goals and interests.* George Bond amply demonstrates the utility of this research premise in this study based upon field research among the Uyombe people of eastern Zambia from 1963 to 1964, a period which marked the appearance of the United National Independence Party (UNIP) on the local political scene. Politics in Uyombe is described as a continuing drama, prominent players including the chief, the royal clans, and the "new men" (relatively well-educated teachers and clerks). These political dramas are often played out on formalized stages, such as royal council, local development committee, and UNIP local branch. Detailed case studies illustrate the content of these social dramas.

For example, a major debate concerned whether or not to establish a special day for the founder of the Uyombe people, and whether such a day would have religious connotations or constitute endorsement of a particular version of the Uyombe foundation legend. The interests of the six royal clans and of the western churches were directly affected. Quite apart from the merits of the issue, the debate also constituted a test of strength between the traditional chief and his administration-appointed, Western-educated deputy chief. All contestants invoked loyalties, appealed to self-interest, made promises, and manipulated tribal "history" in their efforts to carry the day.

Many students of political change who have argued that Third World development patterns involve the centralization of resources will be interested in Bond's treatment of center/periphery relations with Uyombe land. Dependency theorists have argued that, at the global level, the industrialized world sucks resources out of its Third World satellites. Within the satellite, capital and labor are sucked out of the rural hinterland to the principal city. Bond demonstrates the same process at the local level. Muyombe, traditionally the chief's home village, was formerly a moving capital. But in recent decades, Muyombe has become a permanent administrative headquarters with nonmovable brick buildings. The local elite prefer that development projects be concentrated there, but in a recurrent social drama, rural villagers resist this centralization.

How has the drama between rural elite in Muyombe and rural villagers in the hinterland unfolded in the years sinze Zambia became independent? Apparently Muyombe town, not the rural hinterland, has been the principal beneficiary of development projects, although Bond has not been able to tell this story with the rich detail and analytical vigor that graces his account of the 1963 to 1965 period. He returned to Uyombe for a short visit in 1973, summarizing his impressions in a brief five-page epilogue.

Another major theme in Bond's study is that the pace of development slowed with the advent of independence—a conservative coalition consisting of poorly educated local businessmen, successful farmers, and the chief and his ruling clique becoming dominant. According to Bond, the most development-oriented individuals in Uyombe encouraged broader popular participation, organized local self-help projects, and approved loans to those who could use them productively. But they were drawn to the capital city following independence and began appointing local party officials and treating agricultural loans as handouts to the politically loyal rather than as investment in greater productivity.

In 1976, we would like to know how entrenched this tradition-oriented, nondevelopmental, local Zambian elite has become. Have rivals appeared, such as better-educated school leavers, locally posted technical officers, and national politicians developing a local power base? What new social dramas are they playing out? Once again, we regret that Bond was not able to follow up his earlier study in the same locality using the same research technique. His epilogue merely notes that the benefits of central government investment in Uyombe following the 1969 riots flowed largely to the elite who received the loans, used the marketing facilities, and owned the trucks that used the improved roads.

On the basis of his original research, Bond predicted that the chief and his retainers would be an important part of the new conservative coalition. As an anthropologist, he is particularly sensitive to the continuing relevance of traditional structures and values; when political scientists do local political anthropology, they tend to write from the standpoint of the nationalist movement, noting in passing the erosion of traditional authority or its adaptation to changed circumstances. The difference in perspective is a valuable one. Unfortunately, Bond presents very little evidence in support of his contention that traditional authority is an important partner in the conservative coalition, nor does he show how traditional and commercial elites interact. Since Bond was not able to replicate his study, the reader has no way of judging whether "a resurgence of traditional authority patterns" (p. 166) has occurred in Uyombe or not.

Ethnographers and students of Zambian politics will find this book particularly rewarding. Its value for students of comparative political change rests in the author's careful demonstration that political change occurs as individuals define and manipulate reality for their own purposes. However, the period described occurred a decade ago, and students of political change have read other broadly similar accounts published in the first years after independence. *The Politics of Change in a Zambian Community* would be a more exciting contribution had the author been able to do a more detailed follow up comparable to his earlier effort. Astonishingly enough, we social scientists interested in the dynamics of political change rarely replicate earlier studies. If researchers today would do their field work in a community that has already been studied extensively rather than seek areas that have never previously known the researcher's gaze, the comparative study of political change would be enhanced.

THOMAS RASMUSSEN

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Patronage and Exploitation: Changing Agrarian Relations in South Gujarat, India. By Jan Breman. (Berkeley: University of California Press, 1974. Pp. 303. $15.00.)

This thought-provoking monograph makes a significant contribution to the relatively untilled field of Indian agrarian sociology. Jan Breman's aim is to examine the nature of the contemporary relationship between a caste of landlords (Anavil Brahmins) and one of agricultural laborers (Dublas) in two villages in south Gujarat. In pursuing his goal he does not limit himself to observations made during the course of his field work, but enriches his account with historical material in an effort to understand the evolution of the situation which he confronted. The result is an important work in which theory, historical perspective and personal observation are intelligently synthesized.

Breman's theme is the gradual disintegration in recent years of the system of permanent bonded labor (the *hali* system) in the villages of south Gujarat, and its replacement by a situation in which the laborer, adrift in a market economy, sells his labor to a local landlord and becomes the latter's employee. In the process, what had been previously a patron-client relationship, in which both landlord and bonded laborer satisfied certain needs, has also disintegrated, leaving the laborer open to "exploitation" by his former patron, turned employer.

The first two sections of Breman's book are devoted to the theoretical framework and historical illustration for his argument. They are certainly the most stimulating and pioneering parts of the work, but for this very reason are more open to question than others. Breman's thesis requires him to cope with well over a century of the agrarian history of south Gujarat. Such a survey is enterprising, but is certainly too daring an undertaking in view of the evidence adduced in its support. The gazetteers and census reports Breman relies upon must be treated with considerable reservation and tested against archival material, contemporary newspapers, religious tracts, etc. Since the sifting of this material is a task barely begun by historians of Gujarat, one might expect a more tentative approach. It is unenlightening merely to repeat terms thought applicable to south Gujarati society by nineteenth-century British administrators; to describe Anavil Brahmins as 'rulers over the village population' (p. 57) on the basis of such administrators' reports shows lack of historical perspective. Moreover, it is hard to believe that the *hali* system remained static throughout the nineteenth century, yet Breman does not use his historical materials so as to give any sense of period or evolution. The reader is often puzzled about which decade is being referred to.

Breman devotes the final and longest section of the book to analyzing field work material to explain contemporary conditions in south Gujarat. The process of "depatronization" which is the theme of this section is largely attributed to a change in the crop system in the area, which has done away with the need for a permanent supply of year-round labor. As a result the Dubla can no longer rely on year-round and lifelong support, but must do the best he can with the finely calculated monetary payment he now receives for his labor. Previously protected by receiving payment in kind, he is now at the mercy of market forces and has become progressively pauperized and exploited.

The interesting aspect of this section is that the author, like a good Weberian, does not rest content with a purely economic explanation for the change he is studying. Although a new economic development provides the substructure of his argument and he rejects the notion that political initiatives or religious movements have had any real influence in the disintegration of the *hali* system, he penetratingly asks himself what motivated the Anavils to introduce a crop change. This produces the important observation that purely economic considerations such as prices, demand, plant disease and so on have been secondary to "social factors," in particular, to the Anavils' desire to spend less time and energy on agricultural activities. Since sociocultural values or "attitude of mind" are such a crucial aspect of the argument, it is a pity that Breman does not make more than passing reference to the religious and social ideals of both his protagonists. Indeed, throughout the book he unfortunately seems to perceive future economic change and improvement in terms of intensification of the "class struggle" and of increasing violence, and tends to dismiss changes in cultural attitude as irrelevant to such improvements. This is particularly striking in his discussion of the Dublas' position. Though admitting in two brief sentences (p. 119) that religious norms reinforced the Dublas' acceptance of the *hali* system, Breman regards the use of means other than political activism to improve their present positon as futile. He strongly criticizes Gandhian social workers for promoting devotional gatherings and encouraging adjustment to Hindu society rather than what he calls "awareness." In fact, since in the past the Dublas' passivity was molded at least in part by certain religious views, modernist religious movements or protest movements with a strong religious component may be greatly significant; the inculcation of individual self-esteem and thus of the desirability of personal endeavor is certainly not irrelevant to economic change.

This said, there is no doubt that Breman's work represents a considerable pioneering achievement. It is lucidly written, the data and tables being intelligently integrated with the verbal analysis. It makes a cogent case for employing a class analysis in a caste-based society, although perhaps the Anavils are too readily cast in the role of "class enemies." Moreover, the author is too perceptive to be overtaken by the march of events. After a recent visit to the area, he prophetically notes the trend away from electoral politics among the higher castes.

CHRISTINE DOBBIN

Canberra, Australia

Squatters and Oligarchs: Authoritarian Rule and Policy Change in Peru. By David Collier. (Baltimore: The Johns Hopkins University Press, 1976. Pp. xi + 187. $10.95.)

Studies of squatter settlements in Latin America have become so numerous and so repititious in their conclusions that yet one more book on the topic cannot but be received with misgivings. The merit of this short book is to offer a novel angle into an old subject by concentrating not on the attitudes and situation of squatters themselves but on the state's varying policies toward them. Such policies are seen as the reflection of the interests of different classes and groups dominant at each specific stage. Emergence of such classes and their conflict and cooperation are, in turn, a consequence of more basic processes of economic change.

The dominant theme running through this book is one borrowed from Juan Linz's analysis of the Spanish political system: authoritarian governments—such as the ones Peru has had during most of its recent history—are primarily characterized by their effort to limit political pluralism. This effort acquires different modalities according to the groups in control of the State, which give rise to the different subtypes of authoritarian regimes. When successful, an authoritarian government reduces the level of popular mobilization and demand making and increases the degree of control by official agencies. This situation produces a greater degree of autonomy of the State vis-à-vis civil society to be used either in the satisfaction of individual goals or the implementation of a collective blueprint for the future.

A second theme in the analysis of the Peruvian system is the professive independence of official state bureaucracies from the interests of civil classes. Though in the first chapter the author stresses the dependence of squatter settlement policy and government decisions in general on conflict and accommodation between dominant economic interests, in the last he effects a partial reversal of this position. There he notes how the more recent of the four regimes analyzed—the military "revolutionary" government—has achieved a substantial degree of autonomy from previously dominant rural and urban sectors. In this manner, the authoritarian State becomes doubly divorced from civil society: now it not only regulates demand making from below, but becomes progressively inattentive to interests and pressures from above.

With specific regard to squatter settlements, the main insight of the book is one already familiar to students of urbanization in Latin America, but which has never before been documented in such detail. This is the idea that land invasions by the poor and the very existence of illegal settlements are not events "forced" upon the government and urban elites, but rather products of the accommodation between the interests of all groups involved. This accommodation insures that the poor *and* the elites benefit from the outcome. While the goals of the invaders are fairly clear, those of dominant groups are less obvious though no less important. They include the generation of political support for government leaders, the clearing of valuable central land, and, above all, the management of popular demand through use of public land and the granting of juridical titles to it.

Land invasions and the subsequent emergence of huge *"barriadas"* are thus phenomena seldom imposed on the government, but rather permitted and even actively encouraged by it. The particular modality of dealing with the urban poor depends on the particular coalition of interests controlling the government at a given time. The bulk of the book is then devoted to an analysis of these modalities in the context of the overall policies pursued by the four most recent governments of Peru.

In general, this account of governmental policy is noteworthy for its wealth of detail and the obvious familiarity of the author with the Peruvian situation. It makes worthwhile reading for both students of Latin American urbanization and those more particularly interested in the Peruvian political system.

There are, in my view, two major shortcomings in this book. First, in approaching the phenomenon of settlements as an interplay between the elites and the poor, Collier never really develops what is perhaps the most distinct feature of this exchange. Unlike confrontations between employers and industrial workers or between peasants and landowners, the key feature of the situation of urban squatters is that their demands and the reaction to them take place in relation to the State. The urban poor *qua* inhabitants of settlements are only indirectly confronted with dominant economic sectors. Thus the characterization of life in the settlements as an interplay between elites and masses is partly misleading. The exchange and confrontations that take place are less between economic classes in society, than between a large interest group from it and the bureaucracies of the State.

Second, and most important, the characterization of the origins and growth of squatter settlements as governed by the political conveniences of ruling groups is only true in a partial sense. This is a truth restricted to the everyday manipulations and bargaining that take place between the government and the settlers. By concentrating on the specific instance and the particular event, Collier may have lost sight of the more general picture. Ultimately, the breakdown of the regulated capitalist land market, the organization of land invations, and the subsequent intervention of the State are events forced upon the social order by a basic need for shelter shared by vast numbers. That dominant groups have proven flexible in accommodating to this situation does not negate the fact that its existence was not their idea but the consequence of the formal pressure exercised by the impoverished majority.

Only by neglecting this fact, could the author "explain" the origins of squatter settlements in Lima as a consequence of the fuzzy juridical situation of land ownership. Such an explanation forgets that, regardless of what the juridical order is, settlements are a phenomenon common to the entire underdeveloped world. Determinants of land invations and squatter settlements are not to be found in juridical regulation, but in the structure of the existing social and economic order.

Despite these reservations, the book deserves a high rating. It represents a significant contribution to the study of public policy in Latin America. In its particular field—the state and contemporary urban poverty in Latin America—it is undoubtedly one of the very best studies to have appeared in the recent past.

ALEJANDRO PORTES

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Social Mobilization and Liberal Democracy: A Macroquantitative Analysis of Global and Regional Models. By Philip Coulter. (Lexington, Mass.: Lexington Books, D.C. Heath & Co., 1975. Pp. 188. $17.00.)

In this cross-national study, Philip Coulter explores the effect of social mobilization on political democracy. Although several other social scientists, including Seymour Lipset, Deane Neubauer, Robert Jackman, and Samuel Huntington, have investigated this topic, Coulter concentrates on examining certain hypotheses at both the world and the regional levels. The first part of the book analyzes data from 85 countries. The second part looks at four regions: (1) Europe and the English-speaking settler societies, (2) Spain, Portugal, and Latin America, (3) sub-Saharan Africa, (4) Asia, including the Near and Far East. For each region, Coulter posits a distinctive political tradition: secular liberalism in Europe, corporatism in Latin America, communalism in Africa, and a bifurcated mixture of secularism and communalism in Asia. These cultural traditions supposedly intervene between social mobilization (urbanization, education, communications, industrialization, economic production) and political democracy (defined in terms of elite competition, voting turnouts, and civil liberties).

Using multiple regression techniques, the author uncovers several inconsistent findings. Hypotheses valid at the global level may not be valid at the regional level. Contradictory generalizations often apply to different regions. For example, at the world and European levels, says Coulter, the rate of social mobilization has more explanatory significance than the level of mobilization. Yet in Asia the level, rather than the rate, exerts the greater impact on political democracy. In Latin America and Africa the rate and level have about equal importance. Whereas for African countries a fast rate of economic development strengthens democratic political institutions, in Asia rapid development weakens political democracy. So, after seven scattergrams and hundreds of correlation coefficients (simple, partial, and multiple), Coulter concludes that social mobilization does tend to result in political democracy but that the specific effects vary from region to region. The unique historical context of different regions produces contradictory tendencies. According to him, rather than discovering universal generalizations valid for all times and places, the social scientist must be content with region-specific findings. In this view, the "collective subjective meaning" linked to regional political traditions determines the interaction between social mobilization and political democracy.

Although Coulter tries to reconcile the breadth of a worldwide study with the expertise of the area specialist, his theoretical conceptualizations, methods, and explanations for his findings raise several questions. First, while admitting that no regime is fully homogeneous, he never adequately examines the extent to which each of the four regions really has a distinctive political tradition. All regions are culturally pluralistic; to varying degrees, they share several diverse value systems, including communalism, corporatism, and secular liberalism. Certainly, all four regions, not just Africa and parts of Asia, reveal strong communal ties. Corporatism has political significance not only in Latin America and the Iberian peninsula but also in Eastern Europe and perhaps even Western Europe. Latin America as well as Asia may have a dual or bifurcated culture. Coulter probably should have conceptualized cultural values as variables, rather than attributes. Although more difficult to measure than voting turnout or number of telephones per 1000 persons, these values could have been incorporated into the data analysis, not left as a residual category for inferring the unexplained variance.

Second, the methods for measuring the variables demonstrate the same weaknesses characteristic of other cross-national investigations. Economists have faced difficulties finding accurate indicators of such "hard" concepts as gross national product, especially in societies dominated by subsistence agriculture. The measurement of "soft" concepts like civil liberties becomes even more difficult, as does the ranking of countries on these variables. In particular, the placement of some nations on the civil liberties scale seems invalid. For example, Mexico ranks higher than Costa Rica. Egypt receives a lower rating than Albania. Furthermore, the measurement of mass participation combines both voluntary and involuntary reasons for failure to vote in national elections; that is, non-voting may stem either from apathy or repression and legal restrictions, such as rigid registration and residence requirements. Surely, however, the involuntary reasons better imply the existence of a non-democratic political system. Despite these measurement problems, Coulter gives equal weight in his liberal democracy index to the three indicators–elite competition, civil liberties, and electoral participation. Yet the first two factors appear more valid indicators, since except in Africa participation has weaker correlations with civil liberties and elite competition than the latter two variables have with each other. Because of these measurement limitations and the assignment of scores to particular countries, the conclusions remain tentative.

Third, Coulter's explanations for the contradictory findings are rather unconvincing. Despite the hundreds of correlations, he concludes that subjective meaning as embodied in unique regional political traditions is the crucial variable explaining the interaction between social mobilization and political democracy. Unfortunately, however, since he regards these traditions as attributes, not variables, he does not measure them cross-nationally. Other explanatory assumptions also remain unclear. Why does communalism hinder social mobilization and political democracy? Of course, communal groups may stimulate reinforcing cleavages which lead to violent conflict and thence to military repression. Yet, as Cynthia Enloe has suggested, certain communal ties may actually facilitate social mobilization, if not democracy. Coulter sees corporatism as an effective strategy for transcending communalism. True, Latin America has attained higher levels of industrial development than Asia or especially Africa; but may not corporatism, with its bureaucratic tradition, have impeded, rather than furthered, economic development? The causal connections are thus not so clear as the author assumes.

Finally, the book unfortunately contains a few typographical errors. For instance, Table 2–1 has no minus numbers for the betas. Yet the text (p. 25) stresses several negative betas. In Table 7–4 the beta weight for communications and democracy is +3.07; however, the text (p. 110) indicates that it should be –1.64.

CHARLES F. ANDRAIN

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Discretionary Justice in Europe and America. By Kenneth Culp Davis. (Urbana: University of Illinois Press, 1976. Pp. viii + 203. $9.95.)

Do Europeans know something that Americans do not about avoiding unnecessary discretion in administration? This question inspired Kenneth Culp Davis to recruit a handful of European scholars, primarily professors of administrative law, to conduct empirical case studies of administrative discretion in Denmark, France, Germany, Italy, the Netherlands, and the EEC's Commission of the European Communities. This book reports the findings of these eight case studies. Davis provides an "American perspective" on each in addition to his introductory and concluding chapters.

Davis presents three conclusions. First, European possess no special insights into how unnecessary discretion can be avoided. The case studies reveal that Europeans appear to encounter the same problems in controlling discretion. Indeed, examples of astonishingly wide discretion are uncovered. An Italian independent public corporation (the Cassa per il Mezzogiorno), acting in behalf of a ministerial committee, administers a program dispersing some 60 billion lire a year to encourage construction of industrial plants in Italy's impoverished south. No published information exists on who gets how much of this money. No clear procedures or standards for awarding the grants could be found. Applicants receive no justification for decisions denying their requests. Second, Davis suggests that students of European administration and administrative law would benefit immensely from further empirical study of how programs are administered. The heretofore exclusive study of administrative courts' decisions ignores entirely the 98 or 99 percent of administrative actions that never come up for judicial review. If the book and the initiation to empirical research provided Davis' European colleagues stimulates such a movement, his efforts will have made an outstanding contribution. Third, Davis suggests that in at least one important area, the decision to prosecute, the Germans do know something that Americans do not, and that Americans ought to reform their "grossly inferior" system of prosecution.

Most of the eight case studies suffer from shortcomings that limit their value. By necessity, they focus on a few very restricted aspects of administration and fail to provide a strong empirical base upon which to generalize about European discretion. The studies vary in thoroughness and quality; some are superficial. All rely on interviews rather than observations. Bold assertions about administrative discretion often rest on slim empirical evidence. The authors frequently inject prescriptive recommendations and normative judgments in the midst of their analysis. Readers must identify methodological shortcomings and analytical weaknesses because Davis offers little criticism in his commentaries. Students of western Europe will not find the book enhances their knowledge significantly.

Joachim Herrmann's piece on German prosecutors is clearly superior to the other studies. It constitutes the core of the text, both in terms of length (approximately one-third, including Davis' commentary) and substance. It is especially important to Davis, for he cites it as evidence that unnecessary discretion can be controlled and that the American system of prosecution must be reformed. The strengths and weaknesses of Davis' inquiry into discretion and justice emerge most clearly here.

Less reluctant to generalize about the German system of prosecution than Herrmann, Davis observes (p. 60): "The principle governing prosecution for felonies is compulsory prosecution, but the main element in the prosecution of minor crimes or misdemeanors is controlled discretion, not compulsory prosecution." Davis draws a striking contrast to the United States: "the American prosecutor's discretion is consistently uncontrolled all along the line." Both conclusions can be seriously challenged.

The evidence Herrmann so carefully presents permits the opposite conclusion—that German prosecutors exercise wide discretion. Although statutes mandate prosecution in many cases, Herrmann presents numerous examples of discretion. Prosecutors can conclude the evidence is insufficient and halt proceedings; some cases require a determination that prosecution will serve the public interest. Although judges must approve decisions to halt prosecution, they routinely grant it. German prosecutors exercise wide discretion in juveniles' cases (which include 18-year-olds). In misdemeanor and petty misdemeanor cases, they can request a "penal order"—the German equivalent of a plea bargain determining sentence. Prosecutors sometimes discontinue prosecution in such cases when the defendant makes a charitable contribution.

Davis overstates the extent of American prosecutors' discretion. He cites case law and statutes to demonstrate that U.S. attorneys exercise wide discretion, but fails to acknowledge informal but potent sources of restraint, including investigative agencies and the Department of Justice. Studies of local prosecutors find similar informal restraints. If "felonies such as murder, robbery, rape, perjury, and arson are prosecuted if there is sufficient evicence" in Germany (p. 23), the same is true in America. German prosecutors probably have more discretion than Davis suggests, and American prosecutors less. Neither system knows how to shape and control the crucial discretion exercised by the police. The two systems may not be as different as this book suggests.

Davis' conclusions about prosecutors' discretion in Germany and America flow from his formal, legalistic approach. Unlike most legally trained researchers, Davis relies on empirical research. He recognizes that part of "what matters is . . . not what limits the law places on the officer's discretionary power, but what limits are effective *in fact* in the particular case" (p. 4). But in examining what in fact limits discretion, he ignores informal, political constraints and too often takes at face value the content of statutes, formal rules, and court decisions. At times, he advocates clearly legislation and administrative directives to control discretion. But he ignores the politics of policy formation that necessitates ambiguity and the delegation of discretion. Finally, Davis' concern that injustice arises when American prosecutors fail to bring good cases leads him to ignore the opposite phenomenon—the failure to eliminate the many "junk" cases that never should have been prosecuted as felonies.

Readers of Davis' pioneering treatise *Discretionary Justice: A Preliminary Inquiry* will find few analytical or conceptual breakthroughs in this volume. But the book makes an important contribution by further stimulating thought about how discretion can be controlled and how its exercise shapes justice. Political scientists too often ignore these questions. Davis is inviting us to participate in his inquiry into discretionary justice. We should.

JAMES EISENSTEIN

Pennsylvania State University

Coups and Army Rule in Africa: Studies in Military Style. By Samuel Decalo. (New Haven: Yale University Press, 1976. Pp. x + 284. $17.50, cloth. $4.95, paper.)

While the literature on the African military has shifted significantly from an uncritical acceptance of the military's capability to modernize states, scholars still search for an acceptable typology or model which can provide a realistic insight in studying armed forces and society. The purpose of Decalo's book is not only to demonstrate the weaknesses of existing models or typologies but also to suggest an intellectual construct which may overcome such weaknesses. According to the author, 'The purpose of this study is to shift attention away from the discipline's fixation upon the systemic weaknesses of African states and the organizational features of African armies as reasons for coups to the internal dynamics of African military hierarchies, their officer cliques, and corporate and personal ambitions" (p. 3). In his quest, the author examines Dahomey, Togo, Congo/Brazzaville, and Uganda, not only in constructing a typology, but in order to rectify the "dearth of scholarly attention to much of French Africa" (p. 2).

Although a number of important substantive considerations emerge from the author's detailed examination of these states, three are paramount. First, it is clearly demonstrated that the military reflects society. Too often in previous studies, authors have a priori proceeded on the basis that society reflects the military or have presumed that somehow the military is separate from society. Internal ethnic strife, ambitions of politicians, lack of institutionalization of the "rules of the game"—all of these matters are generally reflected in the military of the four states in one form or another. For example, in examining the Congo/Brazzaville, Decalo writes, "From their inception the armed forces reflected the wider societal cleavages in [the] Congo. Ethnic tensions were intense, with the rank and file largely northern (Mbochi, Kouyou), while southern (Bakongo) elements held key positions in the officers corps" (p. 146).

Similarly, in examining Dahomey, the author concludes, "Persistent infighting at the apex of power, competitive ethnic pyramids of personal allegiances, and the keen struggle for control of patronage in conditions of acute scarcity and immense pressures from below have plagued all attempts to establish interregional coalitions" (p. 51). These characteristics are reflected in both civilian and military sectors in the country.

Second, the author shows that military rule is generally no different from civilian rule. In most cases there has been no clear impact on social or economic systems attributable exclusively to military rule. Indeed, military regimes seem to demonstrate the same inadequacies of civilian regimes. On the other hand, the author states that "even though military administration may not be systematically any different from civilian rule the extra-constitutionality of the regime allows it to perform certain actions no civilian regime may dare attempt" (p. 239). This follows the observations of some scholars that there is at least one clear benefit of military rule—that it may do those things that, although not democratic or immediately conducive to beneficial social and political change, may in the long run prepare the ground work for a stable political system.

The third aspect of the author's work, although logically argued and persuasive, raises a few questions. This has to do with his modalities of African military style in political office. This formulation has six categories: "The corporate status of the armed forces, the permeability of civil-military boundaries, the degree of personalist concentration of decision-making authority and coercive power, the satisfaction or non-satisfaction of nonmilitary group demands, the relative immunity of the regime from personality cleavages leading to praetorian assaults, and the active-combatative or passive-reconciliationist approach of the regime to societal issues" (pp. 241–42).

Some of the variables in the typology (as the author admits) are themselves subject to various interpretations and intuitiveness. Moroever, it is not completely clear how the typologies evolve from the assessments of the four countries. For example, the author categorizes the Togolese regime as a civilianized military autocracy, placing into jeopardy his own typology. This categorization suggests some aspects of corporate status and some considerations regarding the permeability of civil-military boundaries. More importantly, the variables within the typology do not provide the necessary analytical insight to understand their significance. One wonders for example, how to apply "the permeability of civil-military boundaries" in the study of military systems. What does this mean in terms of relationship between armed forces and society and the impact on the political system?

From the point of view of model building, the first and last chapters are the most useful, while the middle chapters focus on a substantive political-military examination of the four states. It would have been useful if the author had hinted in the early chapters regarding the organization of the chapters and their culmination into modalities of African military style. Finally, while the author's criticisms regarding the literature on the African military are well founded, it should be noted that recent literature has rectified much of the earlier uncritical examination of the military in modernization. Moreover, some scholars have indeed focused on the internal dynamics of the military. For example, Luckham's work on the Nigerian military (1971) although rooted in social organization theory provides a clear picture of the internal dynamics of the military and the fragmentation within the officers' corps.

Perhaps the major contribution of the book is in its functional perspective—that is, the author attempts to examine the military and its internal dynamics across a broad spectrum of political developments in each state, without artificially categorizing the military into pre-coup, coup, and military rule segments. The author provides a coherent perspective crystallizing the role of the military as an institution (using the term with a degree of qualification) in relationship to other political actors. This type of perspective may be a first essential step toward developing the kind of formulation that Decalo longs to see, but seems to be lacking in earlier literature.

In any case, Decalo's study of the military in Dahomey, Togo, Congo/Brazzaville, and Uganda is an important contribution to the literature on the African military. The author's attempts to provide yet another intellectual construct for the examination of military systems in general provides a unique perspective. While one may argue about the application of such a typology, it is clear that Decalo has produced an extremely useful work that should assist scholars in understanding the military in Africa and in theorizing about African military systems.

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Legitimation of a Revolution: The Yugoslav Case. By Bogdan Denis Denitch. (New Haven and London: Yale University Press, 1976. Pp. 254. $15.00.)

Denitch's study is a refreshing attempt to keep Yugoslavia's political hypertension of the 1970s in perspective. Faced with the chaos of constitutional change, rising nationalisms, party purges, rumors of external dangers, and attacks on "internal enemies," he has moved beyond those prophets of doom who speculate primarily on those forces pulling Yugoslavia apart "after Tito" to the opposite question. What are the forces with a vested interest in keeping Yugoslavia together? Who has a stake in system survival and why?

This approach leads to examining those social strata and institutions that provide an infrastructure for the authority of Yugoslav socialism as practiced by the present regime. Not surprisingly, Denitch emphasizes the main recognized clientele of the Yugoslav political establishment—the industrial working class. He divided intelligentsia into two broad groups, the technical elite and "humanistic" intelligentsia, while he gives the peasantry (which Denitch considers basically outside the political system) minimum time. Institutions include the party (League of Communists of Yugoslavia), the army, the veterans organization, and the Socialist Alliance, which Denitch views much as the equivalent of a primary system in a single-party electoral system. Throughout, he evaluates these political actors and structures in the context of Yugoslavia's continuing struggle to subsitute mass-based, participatory institutions for existing models of socialist society.

Two key concepts dominate this analysis: modernization and legitimation, which Denitch uses to link the Yugoslav experience to the growing body of literature concerned with developing models of revolutionary transformation and to attempts by political scientists to pinpoint the elusive characteristics of legitimacy. Whether or not one agrees that chapter 8 successfully applies Richard Rose's middle-range dynamic model of authority, it is an intrinsically useful way of thinking about complex, often little-understood data. The treatment of Yugoslav self-managing socialism as a new political culture adds to the works of scholars of comparative communism such as Robert Tucker and Richard Fagen for whom political culture has become an increasingly valued analytical concept. Therefore it may interest scholars concerned with cross-systems analysis. To what degree the author's optimism is warranted concerning the rate and permanence of change in the historical political cultures of the region is another matter. At a minimum he has provided the reader with a well argued thesis.

While Denitch is an optimist, he is not unaware of problems in Yugoslavia. He sensitively discusses the difficulties raised by the age homogeneity of the current leadership and the frustrating search of young and aspiring political elites for a rite of passage to substitute for partisan experience. Denitch's analysis of the institutionalization of multinationalism using Yugoslav sources effectively demonstrates how none of the Yugoslav nations has been allowed to be officially "more equal" than the others. Public opinion polls apparently support that official image.

It is on this point that long-term observers of the Yugoslav political scene may feel uneasy. Attitude surveys investigating ethnic relations within Yugoslavia in the 1960s were poor indicators. Most of those surveyed rated national relations "good" or "satisfactory." Ethnic tolerance appeared high; ethnic distance seemed to have declined. By the late 1960s even more favorable prognoses were being made for the near future. No one could have foreseen that in 1971 the leadership of the Croatian League of Communists would be purged for what amounted to the charge of serving as a front for a mass nationalist movement.

In dealing with the "national question," Denitch's analysis becomes uneven and somewhat inconsistent. First, we are told that nationalism is overrated, given more weight by the media than it deserves, that "traditional nationalism has only sporadic and very limited support" (p. 6). Secondly, we learn that most of the Croat political and economic demands of 1970–1971 were later realized (p. 131). Thirdly, in the author's view, nationalism in Croatia in 1969–1971 was "a surrogate for anticommunism" (p. 147). Given that the bulk of Croatian demands subsequently appeared to have been considered reasonable enough to meet, such a judgment seems polemical at best. At worst it leaves the book open to the charge of being a sophisticated apology for purge and repression as legitimate tactics for resolving differences in contemporary Yugoslavia.

Another delicate issue that the book somehow waffles on is that of conspicuous consumption on the part of the Yugoslav middle class, the technical, administrative, and managerial elites. This behavior creates social tensions leading to recurring campaigns against social differences. Denitch first argues such differences are "more apparent than real" (p. 11). He then calls for "a major campaign" against these abuses as essential for the morale of society (p. 183). The problem is twofold. Apparent differences are "real" in that the system treats them as real and they produce resentment or cynicism. Secondly, the strategy of "major campaign" does not work unless it eliminates perceived as well as "real" differences; otherwise the cure only magnifies the political problem by calling attention not only to social differences but to the regime's inability to eliminate them even (perhaps particularly) among party members.

A third flaw, one that the author is perhaps not responsible for, is the scant information on how the new constitution of 1974 changes the political dynamics he describes. To what extent, for example, has it changed the basis of legitimacy?

Yet despite its flaws, this is an important book. It is original, theoretical, provocative, and relevant not only to Yugoslavophiles but a less obvious clientele of political scientists studying revolution, political parties, civil-military relations, or the usefulness of opinion polls. It is well written and a pleasure to read.

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Allende's Chile: The Political Economy of the Rise and Fall of the Unidad Popular. By Stefan De Vylder. (New York and London: Cambridge University Press, 1976. Pp. xi + 251. $13.95.)

Compared to Argentina, Mexico and Brazil, about which a profuse literature has been generated, Chile has been, especially in political economics, a terra incognita. The rise and fall of ill-fated President Salvador Allende has changed all this. As if to atone for past neglect, scholars have taken a new interest in Chile—characterized in part by genuine astonishment like that following Castro's advent in Cuba, in part by a macabre spirit like that following Kennedy's assassination. Unfortunately, the numerous instant experts have produced mediocre work at best. Mostly unwilling either to learn or to listen, they have embarked into vociferous presentations of their views, cases, or explanations (reflecting both the Left and Right and one representing the center) instead of examining the sad but fascinating Allende experience.

Stefan De Vylder's treatise is, in spite of numerous and major weaknesses, a welcome exception. A sympathetic view from the Left, it is generally scholarly, heavily empirical, balanced, and objective. It provides a detached analysis of major economic events during Salvador Allende's reign as well as a good understanding of Chile's economic and political structure. It comprehensively reviews and evaluates the economic policies of Allende's Popular Unity (UP). De Vylder cites the works of such central UP protagonists as Salvador Allende, Jacques Chonchol, Pedro Vuskovic, Amérigo Zorrilla and Orlando Millas. Tables and other material from primary sources provide a vivid picture of Allende's presidency. There is no index, however, and some tables lack sources (e.g., Tables 1.1 to 1.7, 2.1 and so forth.

The book consists of a spotty introductory segment providing background about Chile, an examination of short-term policies and a discussion of the long-term dimensions of Allende's strategy.

The economic and political introduction reveals little new to the specialist but will be appreciated by the reader unfamiliar with Chile (the sectoral analysis is first-rate, the one on agriculture excellent). Its major themes, however, should not be taken at face value. For example, the book suffers from acceptance of the almost universal misconception that Chile experienced "acute economic stagnation" (p. 24) and its achievement "during the present century would be 'secular stagnation' " (p. 9). This view is contradicted not only by a century-long growth rate of per capita income of at least one percent per year, and the more recent boom of 1957–1967, but also by De Vylder's own earlier admission that "Child's *per capita* income has been quite respectable. It was once the highest in Latin America, and . . . is one of the highest income levels in the Third World" (p. 7). Even stronger empirical and analytical objections can be raised against De Vylder's acceptance of the monopolism and dependence themes.

De Vylder's analysis of Allende's short-term economic strategy illustrates as well as reflects the basic contradictions of UP's program. The 1971 policies of economic reactivation through income redistribution are hailed at first as a "spectacular success" (p. 77), but this evaluation is subsequently modified by listing the "heavy–and largely negative–repercussions" (p. 77) that forced the economy "to exceed all its bounds" (p. 78). The massive para-Keynesian deficit-spending policies that promoted consumer, not investment or production, socialism failed because the system "had been created to provide *selective* favors but which could in no way be used to raise the living standards of the Chilean population as a whole" (p. 210).

De Vylder's own assessment of Chile's long-term problems–sectoral *désarticulation* (high productivity differentials) (pp. 10–12), foreign dependence (pp. 13–16), and a monopolistic structure (pp. 17–19)–is in harmony with the Popular Unity's dogmas which attributed Chilean underdevelopment to imperialism-dependency, monopolism and feudalism (pp. 34–39). These simplistic, grossly inadequate diagnoses (subsequently refuted by De Vylder himself) have as their common and unacknowledged ancestry a more than a century-long Latin American chameleon ideology which was born along with independence with the color of "colony-metropolis" domination. This ideology resurfaced during 1930–1945 as the "center-periphery" concept, blossomed into the full-fledged "structuralism" of the 1950s and 1960s and was recently refurbished into the "dependency" theme.

The Chilean road to socialism was a "blind alley" (p. 1). The Popular Unity's "economic strategy as such promised no solution to Chile's basic development problems" (p. 217). "The anarchy of the market was replaced by the anarchy of neither plan nor market" (p. 153). There was not one but an almost infinite number of Achilles' heels in President Allende's program.

The Allende experiment died in its infancy. Was it too anemic to survive or too feared to be tolerated? The answer eludes De Vylder and may never be found. What is certain to De Vylder is that Allende's road was "not to socialism by peaceful means but to fascism by violent means" (p. 1).

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Electoral Politics in South Vietnam. Edited by John C. Donnell and Charles A. Joiner. (Lexington, Mass.: D.C. Heath, 1974. Pp. 198. $14.00.)

The essays in this volume analyze the politics of South Vietnam during the period from the adoption of the 1967 constitution through the 1973 Senate election–the last national election held in that country. The book grew out of a conference of Vietnamese experts sponsored by the Southeast Asia Development Advisory Group (SEADAG). Six of the conference papers (written originally in 1971, but some updated and revised) are included in the book along with an appendix summarizing other conference reports and the general discussions of the participants. In addition the editors have written introductory and concluding chapters and an essay analyzing the final Senate election.

Most authors whose essays are included in the volume, while decrying some practices of the government of Nguyen Van Thieu, agreed that positive political developments occurred in

South Vietnam during the six years under discussion. The military leadership was no longer a monolithic group, civilians were playing a greater decision-making role in Saigon, the government had gained peasant support because it had provided greater security against local communist attacks and because of its land reform program, and by the end of the period an increasing number of South Vietnamese citizens accepted elections as a means of legitimizing succession to power.

In an early chapter Stephen Young examines the compatibility of democratic elections with the traditional political culture of Vietnam. He concludes that in spite of some areas of friction "several electoral developments seem auspicious for the future." In a later essay Young, who worked in villages in the Mekong Delta, discusses improvements in the standing of non-communist village governments resulting from the transfer of planning and some budgetary authority from Saigon to the villages. He also describes the role of the Tan Dai Viet party in developing greater political consciousness at the local level.

Theresa Tull writes of the increased electoral participation of citizens after 1967 even in cities where anti-Thieu feeling was the strongest. Voters used the ballot to replace all but 41 of the 119 lower house members who sought re-election in 1971, and the Buddhists fielded candidates in Senate and lower house elections in 1970 and 1971 after virtually boycotting earlier elections.

Two Vietnamese participants at the conference were men who combine academic life with politics—Nguyen Ngoc Huy, a leader of the National Progressive Movement, and Tran Quan Thuan, a Buddhist activist and national senator. Both men opposed the Thieu regime, but both supported an independent, democratic South Vietnam. In his chapter Huy argues that many of the problems of South Vietnam's politics during this period were essentially the same as those of all more or less free developing societies—"an inexperienced citizenry," numerous weak political parties, and impecunious candidates who felt they must seek organizational support from local administrators or religious groups, thus inevitably weakening their ability to work consistently for national programs. Added to these "normal" problems was the special pressure of the war which carried with it the constant threat of political assassination. On the positive side, Huy insists that press freedom in Saigon had "some real substance and [was] not a charade." He notes that antigovernment candidates won election to national and local offices despite "harassment" by majority activists and that some pro-Thieu candidates won legislative seats strictly on their merits.

The summary of conference discussions suggests that Thuan, while recognizing Buddhist gains in the recent Senate and lower house elections, argued that the Buddhist-supported candidates would have fared still better in completely open elections.

The Vietnamese participants, like most of the American ones, agreed with Douglas Pike that the North Vietnamese government would block overt participation by the National Liberation Front (NLF) in national elections in South Vietnam. To allow participation would be to recognize the independence of South Vietnam which the North could not condone. There was the furtuer practical political consideration that the NLF and its supporters constituted no more than ten percent of the population. Elections would serve only to point up the NLF's lack of popular support.

The book contains much that is of interest about South Vietnam's politics and elections. Its value in the long run, however, may be to remind us that serious students once thought it worth their while to study South Vietnamese elections and to publish their findings. There will be elections in the new Vietnam, but the hundreds of foreign reporters and scholars who used to analyze the politics of South Vietnam will be missing. There will be no reports from harassed but critical local papers or "third force" spokesmen. Only the official press and radio will describe the elections and their descriptions will be of greater interest to students of propaganda than to students of elections.

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Origins of the Modern Japanese State: Selected Writings of E. H. Norman. Edited by John W. Dower. (New York: Pantheon Books, 1975. Pp. 497. $15.95, cloth; $5.95, paper.)

E. Herbert Norman (1909–1957) was the last of the great amateurs and the first of the professionals among western writers on Japanese history. Like Aston, Satow, and especially Sansom, he was a diplomat by profession. But his Japan birth and experience were supplemented by a rigid academic training that led to a doctoral dissertation at Harvard. Published by the Institute of Pacific Relations as *Japan's Emergence as a Modern State* in 1940, just as Japan forced itself upon the world's political and scholarly attention. Norman's work became the point of departure for the scholarship on modern Japan that began to flow from university presses in the years after World War II. Norman himself had only limited time for further scholarly work during a highly successful career in the Canadian diplomatic service that ended with his death while ambassador to Egypt. Despite this, he maintained close connections with the world of Japanese scholarship, and he was never too busy to respond warmly and helpfully to requests for advice.

Like most classics, Norman's *Japan Emergence* gradually became more talked-about than read. John Dower, the editor of this volume, holds that Norman's work was driven from the reading lists by politically motivated "modernization" scholarship which, in response to cold war pressures, wanted to put a better face on Japan's modern transformation, and that Norman was driven to his suicide as a result of McCarthy era Senate hearings that linked his name to prewar left-wing groups. Dower's impassioned 117-page introduction summarizes Norman's thesis, which he charges scholars have suppressed, then goes on to contribute its own distortions in a critique of the 1960s scholarship on Japan. It would be inappropriate to try to deal with all of this here, but a few points may be noted. *Japan's Emergence* has remained continuously in print, from the University of British Columbia Press, for half of the cost of this hardcover edition, complete with a bibliography which Dower sacrificed in favor of a Norman essay, "The Shrine of Clio," and part of an unpublished manuscript, "The Feudal Background of Japanese Politics." Further, Dower's references to the Senate hearings are marred by a poor jumble of discussions of China scholarship, where the term "feudal remnants" was controversial, and Japan scholarship, where it was not. The connection between those hearings and Norman's tragic death in Cairo was far more obscure than Dower's assertions would suggest. And finally the response within the Japanese academy to modernization theory has been complicated by aspects of prewar and wartime controversies which Dower does not mention.

It would be unfortunate if Dower's polemics distracted attention from the works he has brought together, for they provide a welcome opportunity to recapitulate the course of postwar historiography on modern Japan. *Japan's Emergence* was, and remains, a remarkable work, lucid in its presentation, original in its conception, and impressive in its documentation. Among dissertations, only Robert Scalapino's *Democracy and the Party Movement in Prewar Japan* (1953) approximates its power of synthesis and largeness of design. There was no model in Japanese publications for a work of this scope, and its Japanese translation served as standard account and text for some years after the war.

It is also true that the scholarship which Norman helped to stimulate has blunted the sharpness of some of his assertions and disproved others. The sweep of his assertion that the Meiji Restoration was the work of an alliance between lower samurai and merchants was challenged by Albert Craig's analysis of Choshu samurai participation and economic historians' inability to locate and identify the merchants. Norman's political narrative retains its sweep and power, but his thesis does not. His economic analysis has fared even worse, for his chapters on industrialization and agrarian settlement, with their tendency to credit government policies exclusively to plots for power, have not survived the more searching work of Thomas Smith, R. P. Dore, and a new generation of scholars in Japan. What has happened to *Japan's Emergence* is not so much a suppression as a full airing.

Like Mr. Dower and the rest of us, Norman was writing at a particular time and in a particular climate of opinion. This is nowhere more clear than in the "Feudal Background of Japanese Politics," a manuscript he prepared for an Institute of Pacific Relations conference in 1945. With the exception of the strongest chapter, which he published in *Pacific Affairs* in 1944 and which Dower unfortunately decided not to include here, Norman never published the essay. Mr. Dower has chosen to overrule him, and at several points even ignores Norman's own manuscript revisions to restore material he feels important. In this manuscript Norman's search was for antecedents of reaction and chauvinism, and he combined sound scholarship with strong polemic and perjorative coloration. Some of this now strikes the reader as peculiarly anacrhonistic, as when the Tokugawa secret police are compared to Gestapo agents. At other points the statements are so provocative as to provide ideal invitations for examination essays, a use to which this reviewer has indeed sometimes put them.

Even so, one is impressed anew by the breadth and depth of Norman's reading and the skill and grace with which he set forth his ideas. He was optimistic about the Japanese people and Japanese culture, but pessimistic about the shortcomings of Japanese political leadership. The modernization scholarship of the 1960s tended instead to look for ways in which

Japan's old order contained the possibilities or even "prerequisites" for effective response to the challenges of modern knowledge, technology, and organization. As a result the focus has been on preparation in terms of literacy, urbanization, and bureaucratization, and the ability of the early modern state to marshal resources. In contrast, Norman's concern was with the way that same state blighted human spirits and stunted initiative; he was more concerned with what people read than the fact that they could read, and he abominated political leadership for what it did instead of thinking it remarkable that it was able to do it. In influential works that might well have been substituted for his "Feudal Background," Norman also looked for and found a Tokugawa writer who inveighed against the political society of his day, and traced the development of the military conscription system of the Meiji period. In Norman's world there was still a path along which reasonable men could be expected to move in building a modern society, and his objective in studying Japanese history was to identify the elements that had distorted that progress.

One can agree with Dower's passionate assertion that Norman was moral without going on to his assumption that his successors were therefore not. Climates of interpretation change with the times that produce them, and Dower and his colleagues are just as surely constrained by their experiences of the late 1960s as any earlier groups were by theirs. If the "modernization" optimism halted the pendulum swing of earlier pessimism, a number of vigorous young figures have more recently taken it as their challenge to move the argument back again. Had Norman lived, one suspects he would have seen this growth of studies he helped inaugurate with relish and delight, for the seeds of most of these positions, pro and con, are to be found in his pages and crowded footnotes.

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The Abortive Revolution: China under Nationalist Rule, 1927–1937. By Lloyd E. Eastman. (Cambridge, Mass.: Harvard University Press, 1974. Pp. 398. $16.50.)

This book documents, demythologizes, and challenges long-held assumptions regarding the functioning of Chiang Kai-shek's government in its most successful years. Through detailed studies of a few key problems, Eastman reassesses efforts to centralize governmental processes in China by extending local bureaucratic control below the county level, by fostering a leader cult centered on Chiang, and by intimidating independent warlords. Eastman warns at the outset (p. xiii) that his findings are not positive; by 1937 the Nationalist government could only "credibly claim to govern twenty-five percent of the area and sixty-six percent of the population of China" (p. 272). That conclusion alone illustrates the importance of the narrow focus of the work. At perhaps no other period in Chinese history is the risk of generalizing about internal conditions so great.

Four topics subjected to scrutiny will be particularly interesting to political scientists: the problem of corruption, the rise of fascism, the analysis of bureaucracy, and the behavior of Nationalist officials in the context of Chinese political culture.

Problems of defining corruption are discussed in terms of the notion *sheng-kuan fa-ts'ai* (obtain an official position and grow rich). A cynical inheritance from the declining empire, this expression bespeaks elements of truth as well as cupidity, for in both the old and the new China before 1949, the surest route to wealth and power was through government office. However, in old China limits on corruption were defined by a normative consensus growing out of the socialization process of education and examination experienced by most officials. After 1905 this civil service system was not replaced by a similar mechanism for official indoctrination. This theme—the exercise of official bureaucratic power unchecked by ideological or systematic punitive sanctions—is reiterated throughout the book.

Thus Eastman views the growth of fascism within the Nationalist party as the search for a new ideology, the foundation of "a new national culture" (p. 44), as well as a sanction for terrorism and repression. Eastman's account of the organization and activities of the Blue Shirts offers a cross-cultural referent for European models of fascism, many of which are predicated on notions of antisemitism and imperialism. Further, his analysis implicitly discounts the possibility that KMT fascism was an expression of conservative class interests—an argument which prevents the author from distinguishing analytically between "reforms" in public health, promoted by the Blue Shirts, and "reforms" in land distribution, which the Blue Shirts shunned.

Together with the account of the Fukien Rebellion, the chapter on the Blue Shirts presents a wealth of richly documented and painstakingly reconstructed material that sets a new standard for historical treatment of the

Republican era. Eastman then goes beyond historical narrative to argue that the Nationalist officials in this period were a governing bureaucracy lacking a social base (e.g., p. 83) which "became in effect its own constituency, ruling in the interests of its own members" (p. 286). This was only in part due to military weakness, and to a reluctance to engage in mass mobilization. More important, Eastman argues (taking issue with Barrington Moore), was the fact that the Nationalists alienated themselves from both rural landholding elites and from urban capitalist interests which might have provided a common foundation for legitimate government. It is this iconoclastic thesis, along with his concluding discussion of Chinese political culture, that is going to stimulate the sharp dialogue and further research that Eastman expects and hopes will follow.

The pitfalls in analyzing Chinese political culture have already been demonstrated in the controversies surrounding Richard Solomon's *Mao's Revolution and the Chinese Political Culture* (1971). Eastman posits the existence of a "modal trait" in Chinese political behavior, namely "the relative lack of commitment to abstract principles" in favor of devotion to personal or factional loyalties (see pp. 295, 311). The pursuit of private interests in Chinese government unquestionably displays forms specific to Chinese political culture. But it seems more convincing to argue, as Eastman himself does in his concluding remarks, that the pursuit of personal and factional interests can be a modal characteristic of powerholders in any political system, including the present People's Republic, and that ideology—a system of abstract principles—is a necessary tool for tempering the arbitrary pursuit of such private interests.

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Comparative Legal Cultures. By Henry W. Ehrmann. (Englewood Cliffs, N.J.: Prentice-Hall, 1976. Pp. xv + 172. $5.50, paper.)

This book is unusual in many ways. It amasses a great deal of data about the "legal cultures" of numerous societies, past and present, and organizes them along academically acceptable lines. It presents and analyzes the work of a plethora of legal thinkers and social scientists, past and present. And it does all this within the friendly confines of a thrifty 151 pages of text. What is more, it is rife with hypotheses and suggestions for further research. Thus, it would seem that I should be gushing over with praise of it, that I should recommend it, with boundless joy, for use in graduate and undergraduate classes that have even the slightest bearing on the legal system, the judiciary, etc. But I don't.

Having written a book on "comparative judicial politics," a much more modest task than writing one on "comparative legal cultures," I am painfully aware of the exhausting steeplechase that confronted Henry Ehrmann. The hurdles are high; the jumps treacherous. What is more, despite the much vaster topic treated by Ehrmann in this volume, he had half as much space in which to synthesize his material. But the result is precisely what one should have expected: fuzzy definitions, overly broad generalizations, and enough qualifications of the generalizations to rob them of importance and significance. In other words, Ehrmann has attempted to do the impossible: to make a molehill out of a mountain. Well, it can't be done—at least by people. Yes, the generalizations are border-truisms, close to being trivial. The specifics are mainly anecdotal, conclusively inconclusive.

For example, here are some of the eternal truths that an avid reader will cull from the pages of *Comparative Legal Cultures:* (1) There are legal elites that run legal establishments in all countries, (2) There are many variations in their recruitment, their education and socialization, the way they select judges, the degree of lay participation in the processes of the law and the judiciary. (3) It is difficult to say whether these variations (a) achieve the goals of the society for which they were designed; (b) would accomplish them better if they changed their form to one used by another culture. (4) All legal establishments are relatively conservative, oriented towards system-maintenance; all adore the *status quo.* (5) Of course, the law can be (and occasionally is) used to effectuate social change, particularly when heavily pressured through the political process. Yawn.

The problem is that this book is an overly elaborate propositional inventory of many disparate facts and studies that add up to little more than tautology, obvious abstractions, inapplicable statistical generalizations, and the like. Then there are the supergeneralizations that desperately need qualification, but are left exposed without any. Want an example? Okay, the book ends on this upbeat notion, quoting Wolfgang Friedmann: "Everywhere the active and enlightened role of the lawyer in the complex process of social engineering is an indispensable and vital aspect of mankind's increasingly urgent and precarious struggle for civilized survival" (p. 151). Oh yeah? Even the Soviet Union? Nazi Germany? Perhaps they were just some of the "breakdowns" mentioned earlier in the text of the book. Much is passed over like this, left unexamined. Much is overqualified and left to die in pieces.

So, despite the fact that this book is crammed full of interesting facts, it is dull. Despite the fact it has a number of helpful insights, it is confusing and frustrating. Despite the fact that it has a readily understandable way of organizing its materials, it has no consistent set of values, no framework of analysis.

Ergo, I recommend it highly for libraries and graduate students doing highly specialized research on legal cultures. It will not help crusty professionals much. It will neither capture the interest nor captivate the affection of fresh undergraduates.

THEODORE L. BECKER

University of Hawaii

Indonesia's Elite: Political Culture and Cultural Politics. By Donald K. Emmerson. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 303. $14.95.)

Using in-depth interviews of 40 Indonesian elites—20 administrators in the higher central bureaucracy and 20 members of the national legislature—Donald Emmerson examines the nature of religious, political, and ethnic cleavages in Indonesian politics. A primary objective of his book is to discover whether elites "who agree on a matter of political importance also share some cultural or political affiliation or experience in contrast to others who disagree; how these linkages between the content and context of belief actually operate; and what potentials they reveal for elite behavior in cultural politics" (p. 30). To uncover these relationships and linkages, Emmerson pays meticulous attention to the reasons why the elites believe as they do, rather than simply cataloguing the content of their beliefs. This approach provides insights into the dynamics of Indonesian politics far beyond what might be expected from the sample size.

To establish a context for his analysis, Emmerson outlines the historical development of the bureaucracy and parliament and details some of the conflicts cutting across these institutions in the nineteenth and twentieth centuries. Then, as in an Indonesian *wayang kulit,* the historical backdrop is used as a screen against which the life histories of two respondents representing religious and institutional polar types are cast. One is an *abangan* administrator and the other is a *santri* legislator. From comparisons of their religious and institutional experiences and their attitudes toward secularization, Emmerson extracts two major propositions which he later explores in the full sample.

The lives of the polar types suggest, first, that the socialization and recruitment of the bureaucratic elite have tended to make them more homogeneous, cosmopolitan, and powerful than the parliamentary elite. And, second, largely because of these historical and institutional advantages, members of the bureaucracy relate more positively to their changing environment, whereas legislators relate more negatively because they are less powerful and more culturally heterogeneous. These life histories also suggest that cultural minorities are apt to see social changes as threatening in favor of the majority, reinforcing strong in-group identity and estrangement from the wider community.

Emmerson addresses the two propositions in the full sample by examining the background characteristics and attitudes of the elites. An analysis of family status, perceived paternal impact, strength of ethnic and religious affiliation, kind of education, and associational affiliation supports the first part of the institutional hypothesis that the administrative elite is more homogeneous, cosmopolitan, and dominant than the parliamentary elite, but provides only mixed evidence that cultural identities are strengthened by elite experiences underscoring minority disadvantages. Bureaucratic and legislative elite differences in attitude conditions such as anomie, tolerance, empathy, conflict, and deference toward authority clearly support the second part of the hypotheses that cultural minorities are more negative and defensive than cultural majorities. Cultural minorities "were less inclined to trust leaders or tolerate alternative religious truths, more inclined to see extreme conflict and anomie in the social environment, and less likely (Christians excepted) to support empathy as a social priority" (p. 218).

The results of the sample findings are used in a concluding chapter to evaluate political events of the 1970s to determine if the policies of the government are mitigating the cross-cutting cleavages in Indonesian society and politics. The picture that emerges is not altogether optimistic. Although the new order in Indonesia has attempted to depoliticize cultural conflicts, is has only been partially successful, and in the process has unwittingly contributed to increased conflict between social classes.

While these conflicts are unlikely to lead to revolution, there is a possibility that in the short run previously cross-cutting cleavages will mesh in a way to create greater dislocation and instability. Certainly, recent events such as the riots during Prime Minister Tanaka's visit to Jakarta strengthen this interpretation. In the end, regardless of which elites are in power, the prospects for millions of Indonesians is continued poverty and dependence.

This is an excellent book. Emmerson has combined a sensitivity and understanding of Indonesian society with modern research techniques to produce a work of high scholarship. His research will be of interest not only to Indonesian scholars, but also to those interested in elites and in comparative research methodology.

ROBERT L. YOUNGBLOOD

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Cabinet Government in Australia, 2nd ed. By S. Encel. (Victoria: Melbourne University Press, 1974. Pp. 255. No price indicated.)

The first edition of this useful work was published in 1962. This second edition is welcome. It constitutes a thorough revision of the original by adding a considerable volume of new material about Australian national government up to 1972 and excluding most of the earlier references to government in the Australian States. The bulk of the work has been reduced from 350 pages and the style of presentation has been tightened to provide a far more penetrating examination of processes of adapting the Westminster-style cabinet government to the Australian federal arrangement over 71 years.

Encel's technique is predominantly historical. He views national government with Cabinet in the foreground and covers his territory with broad explanatory sweeps such as "Constitution and Crown," "Cabinet and the Party System," "Responsibility and Accountability." It is within these standard perspectives that he adds a substantial amount of new material with competent insights into "Secrecy," "Corruption," "Cabinet Parliament and Mass Media" and "Control of Administration."

At the core of this study is Encel's fascination with the record-breaking Prime Ministership of Sir Robert Menzies and his subsequent influence on the style of government in Australia. He writes of the Menzies years as "The Ming Dynasty 1949–1966" and adds a new chapter "The Ming Dynasty 1966–1972" to cover the period of Menzies's Liberal Party successors Holt, Gorton and McMahon. Each held office as Prime Minister, but working in the wake of Menzies, their prime-ministerial days were brutish and short.

Encel, it appears, limited his revision for this second edition to events up to December 1972, just as the Labor government returned to power under E. G. Whitlam. As a result, there is scant mention of the dramatic changes Labor invoked after 23 years in Opposition. They presumably wait for Encel's third edition. Indeed, such has been the pace of political events in Australian government since December 1972 that Encel's third edition is now needed, before the second is barely established.

This is not to say that Encel's second edition is without usefulness. Quite the reverse. With Labor tipped from Office by the Governor-General in favor of a Liberal Government in November 1975, and with the Australian electorate's confirmation of that new government at the polls in December 1975, the old norms of Cabinet Government are being reviewed. Some outspoken Labor Ministers had contended that the norms of Cabinet Government were euphemisms for vesting power in the hands of permanent officials (a thesis they would *not* have gained from Encel). Liberal Ministers on the other hand generally find the old norms of Cabinet Government to be the essence of responsible parliamentary democracy. They will, therefore, find it useful that Encel has collated and examined these norms systematically. His second edition will be used increasingly as Ministers search into our past seeking precedents to give legitimacy to their present.

Another merit of Encel's work (and this applies to both editions) is his insistence that it is misleading to interpret Australian government through the preconceptions of the so-called Westminster Model. Whereas his thesis raised eyebrows when he first advanced it in 1962, it is now widely accepted. It has taken Australians a long time to discard their dependent mentality in evaluating government. But as valuable as such independence of mind is, no one, including Encel, has yet written a work dedicated to it. Paternalism in British education has had a profound influence upon Australian scholarship.

So despite his warnings, Encel spends a lot of space in his second edition wrestling with the relevance of the late R. H. Crossman's thesis on Prime Ministerial Government in Britain for the Australian setting. He forgets to stress that Prime Ministerial Government can mean many things to many people. He finds, after studying the governments of Menzies, Holt, Gorton and McMahon that "the predominance of the Prime Minister became such that the ministry could readily be taken as a one-man show, where ministers were no longer responsible to Parliament but to the Prime Minister." Yet, despite that conclusion, Encel believes that Crossman's thesis is deficient for Australia because "the political predominance of the Prime Minister [in Australia] was not matched by a corresponding control over government policy" and that "the great increase in the size, strength and influence of the government bureaucracy was not matched by corresponding developments at the political level."

By and large this is where Encel leaves readers of his second edition. And, as mentioned before, he also leaves them at the point when the Labor Party came to power in December 1972. So it is to be hoped that his success with this second edition does not delay his third, for we need his analysis of the Whitlam years of 1972–1975.

G. S. Reid

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The Shambaa Kingdom: A History. By Steven Feierman. (Madison: The University of Wisconsin Press, 1974. Pp. 235. $12.50.)

The evolutionary study of human political systems requires that we obtain information about all possible types of politics, classify them, and then explain theoretically the relationships of each type to others in the classificatory schema. Certain kinds of information are most useful in this kind of research. Evolutionary changes are most often inferences and hypotheses based on correlations. Therefore, an in-depth study of a changeover from one major type of political system to another is of crucial importance for checking older theories and developing new ones. This difficult but rewarding study of the East African Shambaa kingdom, its development from a prestate system, and its subsequent decline, does provide such information and is therefore a major contribution to our understanding of the evolution of centralized governmental institutions.

Feierman's study describes how a group of stateless societies were united into a centralized state in northeast Tanzania sometime in the late eighteenth century. Original inhabitants in the area were joined by two sets of in-migrants whose customs of lineage solidarity made it difficult to integrate with the local people. Into this situation of increasing conflict came a migrant leader who created marriage alliances for himself among a host of the local indigenous peoples and then, using these as a means of military unification, subjugated the two most powerful towns in the area. Later his son took over a third town. Some of those resisting the rise of Shambaa tried to centralize as well but failed. Finally, a large area was united, tributes collected by the central rulers, and redistributed to the supporters of the central rulers—creating an upperclass elite at the center of a coercively united emergent state.

Feierman shows in detail how the state organization modeled itself on previous lineage-based village political culture. The relationship of the king to his subordinate chiefs was that of father to sons. Indeed, kings married women from each district, then appointed a son of each mother as the local chief. The bureaucracy was made up of centrally based royal relatives and clients of the king, as well as mother's brothers, especially in the districts. Lineage ties to the male relatives of a king's mother were important throughout the king's life, especially in succession struggles. In effect, this is the usual East African pattern of the mother's kin group acting as a political faction who back their candidate for office, then reap rewards when he succeeds. Feierman adds much to our previous knowledge of such political structures by showing us how they devleop and even how the actors view them. Thus, one monarch tried to stop the interregnum fratricidal struggle by installing his own grandson before he died. Ultimately the sub-chiefs revolted successfully and the kingdom split up; a revolt of a self-proclaimed oppressed group followed, in which the oppressed, stimulated by the Arab slave trade on the coast, became in turn the oppressors.

These are but the bare bones of a fascinating account. Feierman is both a trained anthropologist and historian. This historical work is to be followed by another on the political culture of Shambaa. Unlike much African history, this one gives evidence of a deep and broad understanding of the indigenous society, its culture and political process. The author's accounts of organizational forms, of ritual expressions of political values is rich and convincing. The historical methods are complex. Most often Feierman uses oral history of local events, localized lineages and villages. Such materials can be cross-checked across informants, and sometimes with written sources. For the earlier history, the author analyzes the founding mythology of the royal dynasty. To extract cultural values and political norms from the literature is one thing; however, to extract actual events is another, and here the author is less convincing, although it should be noted that he himself notes the difficulties in using such techniques.

Regarding more general theories of state formation the book supports notions of population pressure as a causal force stimulating intergroup conflict and the emergence of more complex political forms. This work adds to existing literature by showing how different types of pre-state socio-political institutions can intensify conflicts when peoples are thrown together in a circumscribed area. The widely accepted theory, used by Gluckman and others, that long distance trade explains the emergence of states, especially in this region, has been discredited by Feierman. The reader knowledgeable about theory can find in the book clear support for and refutation of a whole series of hypotheses about state formation. Feierman does not present any particular theory, but that is not his task. He is, in this book, an historian struggling with the unwritten history of a non-Western people in which cataclysmic changes have occurred. His reconstruction of these events and their causes is generally convincing and masterful. Only with such works will we ever be able to fully appreciate the various trajectories by which events and social forces have operated on pre-state systems to select and retain trends leading to the complex political systems that finally took over in human history.

RONALD COHEN

Northwestern University

Blue-Collar Stratification: Autoworkers in Four Countries. By William H. Form. (Princeton, N.J.: Princeton University Press, 1976. Pp. 335. $17.50.)

In a refreshingly candid preface to this work William Form urges researchers to avoid moderate-sized comparative studies of the sort represented by this book. Studying four examples, he states, provides neither the depth of a case study (or comparison of two cases) nor clear evidence of trends. This is a fair assessment of Form's analysis of the effect of technology on working class political attitudes. Nonetheless, this is an important work which will be of great interest to students of labor politics, political socialization, social stratification, and others.

Form's method is to analyze technology at three levels: (1) level of industrialization in society (using Italy, India, Argentina, and the United States), (2) level of technology in the industry (automobile industry), and (3) range of complexity of jobs within the industry (assemblers, semiautomatic machine operators, inspectors, and skilled craftsmen). Since technology of the industry is a relative constant in the study, Form uses his data to show whether (1) individuals with similar job complexity hold similar attitudes across cultures or (2) whether individuals within the same culture hold similar attitudes for jobs of varying technological complexity.

Quantitative techniques used in this study are confined primarily to simple tabular analysis using chi-square significance and corrected contingency coefficients to indicate association. In addition some log-linear interaction analysis is performed in the discussion of stratification. Also an anomie scale of the Guttman type is emphasized. This scale has a coefficient of reproducibility somewhat below the conventional cutoff of .9 for acceptance; no coefficient of scalability is reported. Methodologically the main fault is overstating in conclusions weak or ambiguous statistical tendencies (e.g., at page 121 Form states that the majority in all factories except the Indian plant preferred the manufacturing sector for a work place; actually, Form's data show the United States is an exception as well). Throughout there is the survey analyst's superficial acceptance of worker responses at face value (e.g., in the same example Form treats preferences for farm vs. factory machine work as an indicator of preferred work milieu; he fails to consider the obvious connotations of these terms for income level and national differences).

The superficial reading of worker responses to questionnaires is particularly striking in the discussion of Italian labor. Based on research completed at the beginning of the 1960s, Form's analysis is premised on FIAT's allegedly successful campaign to reduce left-wing influence within the plant (p. 41). His discussion of worker job satisfaction contrasts with virtually all accounts of the 1972 workers' take-over of the Turin FIAT plant he studied: "Hundreds of mass picket lines, red flags, workers' courts at all gates, blockades of finished products, imprisonment of managers, well-organized settlements of accounts with foremen and guards, systematic terrorism against strikebreakers" (Potere Operaio, 1973). A worker in 1972, from Form's Mirafiori FIAT plant in Turin was quoted as saying of this event, "In our occupation the factory is a starting point for the revolutionary organization of workers."

Discounting radical rhetoric, Form's discussion of Italian auto workers at FIAT emphasizes worker job satisfaction, integration, adaptation, and stability. This discussion is totally inadequate to understand what later happened at the enterprise Form studies. It provides no basis for understanding the spread of thousands of worker delegate councils which was part of a resurgent workers' control movement in Italy. Can it reasonably be supposed that the FIAT workers' movement of the 1970s had no basis in the social consciousness of workers a decade earlier? Or is it that survey techniques document something too superficial to aid in understanding sources of social ferment latent in the multiplicity of contradictory beliefs held by workers and other citizens? It is this attempt to rationalize belief, to structure it, which differentiates Form's sociology of work from more profound analyses. (To note a preferable method this reviewer would cite Sennett and Cobb's *Hidden Injuries of Class*.)

This is not a mere failure of ability to predict. It is a methodological failure. As long ago as 1969 sociologists such as Ehrlich and Tarter published articles in prominent sociological journals showing that in most circumstances attitudes expressed on surveys are *poor* predictors of behavior. Form chooses to ignore this problem as if it were not real. In fact, this problem is central to the book. It is a commentary on the state of the art that a prominent sociologist could secure massive financial support from leading public and private funding sources for a study of this sort. It is still conventional operating procedure, but it is time for the discipline to evolve beyond the naive enthusiasm for survey research of a generation ago.

To end on a more positive note, Form's study does generate a wealth of new data that will feed theoretical speculation for some time to come. Form is an insightful author with enough experience in this field to have designed the present research to anticipate common criticisms of his earlier work (e.g., questions pertaining to job satisfaction are much more detailed and approach this concept from a number of dimensions). He also has the vision and ability to relate his case studies to the most interesting and sweeping theoretical issues in social science. The discussion of technology and its relation to anomie, participation, and stratification cannot be discounted even though it is, in the end, speculative.

G. DAVID GARSON

Tufts University

The Brazilian Peasantry. By Shepard Forman. (New York and London: Columbia University Press, 1975. Pp. 319. $13.50.)

This book is both a polemic and a work of social science. The polemical section derives from the era in which it was conceived and written, the early 1970s. A number of North American social scientists who had conducted research in Brazil prior to the "Revolution" of March 31, 1964, openly and vehemently attacked the military regime then, and now, in power. A good deal of the work published about Brazil from that period reflects their righteous—and rightful—indignation at the end of the Brazilian populist experiment in 1964 and the imposition of a growth-oriented economic model and an authoritarian political system.

Typical of that approach is the author's comment that "an entrenched military dictatorship has bridled the masses, and seeks to harness them to the national purpose of Transamazonian development" (p. 2). In truth, the transamazonian highway and settlement program today is neither panacea nor threat, but merely another expensive military bibelot, albeit of continental proportions, that probably says more about the internal politics of the Army in the Medici government (1969–1974) than of insidious plots to further degradate the Brazilian peasantry.

Moreover, the frequent references to the continued impoverishment of the peasantry, stated in tones that reflect the author's personal shock and repugnance for the regime in power, detract somewhat from the clarity and value of his analysis. For example, "the peasant problem is not a northeastern problem. It is a national disgrace" (p. 9). Forman's social science objective, heavily laced with autobiographical comments about his travels and work in the Brazilian northeast in the 1960s, is "to know how the peasantry has been rendered a part of the Brazilian social system and not with the actual workings of the totality itself" (p. 3).

In his historical survey of the peasantry, the author correctly refuses to employ feudal models to explain the dependency relationship between worker and master. The landlord-tenant/patron-dependent relationships characterizing the peasantry, he argues, derive directly from the labor arrangements reflecting a commercial export-oriented plantation economy. The agrarian crisis that characterizes Brazil today is directly related to the unequal pattern of land distribution and the preferred position of the latifundia in competing for land and labor among the different components of the rural economy. Forman provides a very useful summary of existing data about landholding patterns and types of rural labor and cogently explores the "asymmetrical socioeconomic bonds that characterize Brazilian peasant society" (p. 75).

Forman's imaginative analysis of the "sociology of the marketing system" (p. 90) in which he constructs a typology of marketplaces and relates these to marketing patterns in general is perhaps the most useful part of his book. His discussion of the "panaceas of development" (p. 115), the Brazilian government's programs to alleviate peasant poverty, is a useful, if pessimistic, overview of the various approaches taken in recent years by different governments. Unfortunately, his analysis of the rural political process provides mostly secondary information about politics and a review of available material about the masses. In his concluding chapter, the author synthesizes social protest movements and popular religion, phenomena long recognized and discussed as uncommon features of rural Brazil.

The lurking hope among some social scientists that the peasantry, downtrodden and sullen, will spark a revolution in Brazil is not supported by the evidence in this book, regardless of the author's normative preference. That the Brazilian lower class—rural and urban—has carried the heavy burden of the "economic miracle" is indisputable. That the military-authoritarian political system functions to preserve the traditional (and repressive) power structure in the rural areas is unquestionable. Given these realities of contemporary Brazil, widely known and accepted, the author's considerable talent and recognized authority might best have been devoted to a less emotional and more sharply focused analysis of peasant society in today's Brazil.

RIORDAN ROETT

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The Politics of Economic Despair: Shopkeepers and German Politics 1890–1914. By Robert Gellately. (Beverly Hills, Calif.: Sage Publications, 1974. Pp. 318. $15.00.)

This study of the economic fortunes and political activities of retail shopkeepers is a welcome addition to the growing number of books on pressure group politics in Wilhelmine, Germany. It illustrates the opportunities (and dilemmas) of a group that is neither prominent nor powerful enough to affect national politics, and it explains how such a group could ultimately come to defend, not its own interests, but a status quo in which it had no stake.

Germany's small shopkeepers became politically active after 1890, when they thought their economic and social position threatened. Until then, they had profited handsomely from a virtual monopoly in a retail market that expanded rapidly as the country underwent industrialization. But big department stores and consumer cooperatives emerged to undercut this monopoly by what the shopkeepers were quick to denounce as "unfair competition." At the same time, the shop employees became militant, demanding shorter hours and better pay. The shopkeepers responded by organizing themselves into pressure groups, first locally, then at the state, regional, and national levels. This groups mounted publicity campaigns, arranged for discount and bulk purchasing, and lobbied the government to curb the power of the big stores and cooperatives. But because they could never align their aims and their strategies, and because they kept competing for the loyalty of a small number of supporters, these groups accomplished little beyond confirming the shopkeepers' dire view of their economic position. These frustrations, combined with growing anxiety about the future, eventually caused the shopkeepers to abandon their avowed political neutrality. After 1900, they became part of the socalled *Mittelstand* movement, an amorphous amalgam of shopkeepers, artisans, and white-collar workers. They backed the conservative parties that promised them reforms and adopted these parties' nationalist, antisemitic, and antisocialist views. Ultimately, in a final bid for political power, the shopkeepers made common cause with the agricultural and industrial elites despite conflicts of economic interest, and these elites enlisted the shopkeepers as allies in defending the established order against the challenge of the burgeoning social-democratic party. By 1914, the disaffected shopkeepers and their "economic despair" had won national recognition—though their expectations for relief remained unfulfilled.

Gellately is at his best describing the political mobilization of the shopkeepers and the astounding proliferation of their organizations. He examines the various kinds of organizations with much care, comparing their leadership and composition, reviewing their conflict-ridden and erratic behavior. He shows how these organizations, which were products of discontent and were devised to overcome this discontent, became instruments not of revolution but of adaptation to existing conditions. In treating these conditions, however, and in trying to place his particular subjects into the general context, he runs into trouble. He adjusts his findings to the currently fashionable interpretation of Germany's prewar politics instead of using them to reassess that interpretation. He also fails to come to grips with the political functions these *Mittelstand* pressure groups served—their usefulness in preserving the status quo, their role in defining the purposes of their allies as well as their constituents, their relative influence among a host of rival organizations. And a final complaint: the writing is pedestrian and often imprecise; the subject deserves more rigorous attention.

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The Soviet Image of Utopia. By Jerome Gilison. (Baltimore: The Johns Hopkins Press, 1975. Pp. 192. $9.00.)

In this slender volume, Jerome Gilison turns his attention to a neglected area in communist studies: the examination of Soviet thinking on the transition from socialism to communism and on the future of communist society. Relying primarily on a close analysis of writings by Soviet ideologues, the author provides a detailed account of the issues pertaining to the communist heritage of utopian ideas, the passage from the current historical period to the final stage of communism, a description of communist society, and the traits of the "new communist man." The principal usefulness of this study thus lies in the identification of the major concerns and problems under scrutiny by the Soviet specialists and the exposition of their often inadequate responses. Gilison's work itself can be said to suffer from a similar weakness; it is highly successful in raising interesting questions and issues, but less so in providing explanations to the gaps and contradictions it reveals in Soviet "utopian" scholarship.

Gilison maintains that a key to the understanding of Soviet politics rests with a comprehension of the ideological premises and objectives stressed by each communist regime. Avoiding the pitfalls of the long-standing but unresolved debate on the role of ideology in Soviet politics and society, he argues persuasively that the emphasis on the utopian aspects of Marxism-Leninism during the Khrushchev period can be traced to the political upheavals of those years. De-Stalinization, the Chinese "Great Leap Forward" challenge and the increasing complexity of modernization produced such dislocations in Soviet political life that a new spirit was required to revitalize the Party organization and its cadres. The solution was to infuse the system with the optimism of a rapidly arriving millenium. The attainment of this communist utopia, however, depended on the propagation of "correct" policies and behavioral norms facilitating the transition to the final historical stage. Under the circumstances an attempt was made to bring political and social activities closer to the standards of utopian communism. This link between ideology and reality was evident in the Khrushchev regime's references to and bravado about the not-too-distant arrival of communism. The final victory of Soviet socialism was to be expedited by the undertaking of policies designed to facilitate the passage to utopia.

This analysis of how Khrushchev's approach influenced the ideological debate about communism and the practical considerations about the transition period is most comprehensive. It is unfortunate, however, that the study does not extend beyond the Khrushchev period except for some scane references in passing and an all-too-brief epilogue (pp. 182–87) on the Brezhnev-Kosygin regime. Without blaming the author for delays suffered in scholarship and publication, the reader is bound to be frustrated by the lack of examination of current Soviet views. As Gilison himself points out, the urgency attached to the achievement of communism under Khrushchev has been considerably diffused by his successors. One is left to speculate about the influence of the present leadership on ideological writings concerning the transitional period and its final outcome.

The underlying theme of this study is that a substantial conflict exists between communist utopian ideology and Soviet reality. The unsuitability of initial Russian conditions for the implementation of the Marxist vision and the growing conservatism of the Soviet regimes have presented Soviet theoreticians with the considerable problem of trying to reconcile actual conditions with the communist dream. Attempts at resolving this problem have in turn resulted in various reinterpretations and contradictions in Soviet literature. Gilison's discussion of the issues which have caused greatest difficulties for Soviet specialists are presented in chapters on the transition, the future, and the new citizen, although the most important questions transcend these divisions.

The principal issues concern the relative importance of objective and subjective factors in bringing about the final phase of communism, the need for either material superabundance or human will in eliciting proper behavior from citizens of the utopian society, and the tensions posed both by demands for individual self-fulfillment and personal freedom and the requirements of societal needs and collective security. Gilison's analysis of these problems leads him to the view that a doctrinal paradox exists between voluntaristic and environmental factors. The Soviet view is that both economic abundance and human consciousness are necessary to reach and sustain the communist society. The author, however, claims that these conditions are two distinct models, each of which is a sufficient condition for communism. This because he tends to look at the achievement of communism in terms of incentives; that is, on the one hand, in the absence of human consciousness, material abundance provides incentives for the millenium and on the other hand, with proper consciousness, no material incentives are needed. The issue, however, goes beyond the contributions of individuals to society; it is concerned with society's contributions to each individual. Consciousness may be necessary to assure individual activities in favor of the collective, but material abundance is required to fulfill the goal of "each according to his needs."

This weakness is a symptom of a mere general problem in the study. While the author's exposition of specific points is most comprehensive and offers a good base for understanding discrete issues, it does not go sufficiently beyond an examination of the Soviet theoreticians' positions on individual topics. It would have been helpful if Gilison had provided us with a mere cohesive analysis of the subject by establishing better linkages between the various issues and placing them in a broader historical perspective.

JACOB BIELASIAK

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Social Service Budgets and Social Policy: British and American Experience. By H. Glennerster. (New York: Barnes and Noble, 1976. Pp. 272. $17.50.)

This book will prove useful to those interested in a variety of subjects from comparative politics to budget reform. Focusing on education, health, and social security spending in Britain and America, Glennerster describes the attempts in these fields to initiate forward planning, output budgeting, and program evaluation and examines their relative successes and failures in these two settings. Explanations for differences in national successes devolve upon economic, political, and program variations.

Chapter 2 reviews two prevailing models of government and decision making—rational comprehensive and pluralist—and argues that both Britain and the United States have been moving toward a synthesis of the two. The tension posed between these two models is familiar. Centralized planning processes are usually closed and frequently isolated from important group participation, while pluralist forms respond to the pressures of powerful groups and ignore legitimate questions of long-term resource allocation. The central component of a synthesis seems to be an opening of the planning process to allow the participation of various groups and interests. Chapters 3 to 5 explore, respectively, the economic, program, and political contexts of social policy making. Remaining chapters describe the experiences of each country with forward planning, output budgeting, and program evaluation.

This book fills an important gap in our knowledge about budgeting and spending processes. Several generalizations certainly enhance our limited comparative knowledge of budgeting and budget reform; and several specific arguments point toward previously unobserved differences in American and British experiences: Forward planning and public expenditure control were promoted in America under the pro-interventionist Johnson administration while in Britain those reforms were stimulated by a desire to constrain the growth of public spending. Program accounting, which groups activities according to objectives and aggregates relevant figures across units, was less relevant to British departments which, unlike their American counterparts, were organized along functional lines; administrative unit budgets were, in a sense, already program budgets in Britain. After considerable immersion in cost-benefit/cost-effectiveness studies, social service administrators in America began to turn to applied social scientists for program evaluation. In Britain, where internal program evaluation simply never took off and large pools of available social scientists do not exist, scrutiny of social programs in cost-benefit terms rarely occurs.

Glennerster's comparative observations are interesting, enlightening, and important. Yet, his explanations for these differences are conventional and disappointing. The study of politics and policy making is no longer in its infancy in which federalism, separation of powers, and undisciplined parties "explain" America and unitary government and strongly disciplined parties "explain" Britain. These constitutional attributes constitute, nearly exclusively, Glennerster's political explanation for observed differences in national budget reforms. PPBS, cost-benefit analysis, and public expenditure control are complex methods and tools of decision making; and their successes and failures of implementation are only remotely tied to broad constitutional issues. When over 90 percent of a financial document is made up of uncontrollable spending, neither independent Congress, contentious president, nor responsible Cabinet will change it much—regardless of how much program budgeting or cost-benefit information and analysis is available.

A second limitation of this work is its insensitivity to technical issues of PPBS and cost-benefit analysis. Given the now voluminous "how-to-do-it" literature, the methodological complexities of program analysis and cost-benefit analysis can no longer be separated from broader explanations of successes and failures of those reforms. General observations to the effect that "their outputs were difficult if not impossible to measure" (p. 189) are simply not enough. The gap between minimum subsistence income and existing welfare payments is a valid quantifiable measure on which to base evaluations of public welfare programs. Disease incidence statistics are appropriate for evaluating various disease prevention programs. And, if efficiency is one criterion, then cost-benefit calculations of diseases prevented per dollar or per pound are useful guides for decision making. Neither the subtleties of measurement problems nor the complexities of alternative calculations are treated in more than a surface manner in this work. Quite a few results of public social programs are now quantifiable and measurable; and scholars who fail to remain abreast of developments on issues of measurement and mathematical analysis will not do their subjects justice. The technology—if not its use—is expanding rapidly. In the face of that expansion, casual references which despair of its limitations in very general terms seem less and less intellectually satisfying.

Third, the book would benefit from substantially greater documentation. We do not know how the author knows what he apparently knows. Participant observation and government documents were presumably the two major sources; but the level of generality often exceeds what could have been plausibly gleaned from these sources. Were interviews conducted? With whom? Such documentation would be particularly helpful for the American case.

Despite these analytical and empirical limitations, the book is a must for students of comparative public policy and public budgeting. The author has done much footwork in tracing budget reform efforts in different national settings—an always difficult problem—and many of the observations and arguments are new and interesting. This is one of those books, however, which the reader follows intensely for information but looks elsewhere for explanation.

ANDREW T. COWART

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Planning, Politics and Public Policy: The British, French and Italian Experience. Edited by Jack Hayward and Michael Watson. (London: Cambridge University Press, 1975. Pp. xiii + 496. $37.50.)

The message of this book can be quickly stated: planning in liberal pluralistic societies is certain to fail unless those who wield formal political power are deeply committed to making planning work. If the experiences explored here are any guide, that kind of commitment is usually wanting. Moreover, a strong *political* commitment to planning will likely remain a necessary but insufficient condition for success.

Why? The answers lie in these dreary stories about the evil fate that overtook central, sectoral, regional, and urban planning schemes in Britain, Italy, and France. Successful interventions are either associated with extreme economic crises that require dramatic attention, or they remain (like glowing references to French planning) as mysteriously unexplained as the various European references to the *congiuntura.*

Failures and disappointments can often be ascribed to conflict over ideology. The political parties (including those on the noncommunist left) that have dominated the national politics of these countries since World War II are not committed to radical social and economic transformations. This being so, attitudes toward planning will be ambivalent and contradictory; comprehensive planning schemes will not get very far from drawing boards; anything that smacks of compulsion will be roundly condemned; and (as in the French case) a studied form of drum-bearing and exhortation will be confused by others (some of them Frenchmen at that) with the rational, coherent, central direction of the economy.

The authors also delineate some striking structural obstacles to planning, national and comprehensive, or sectoral, regional, and local. Tensions among central, regional and local government authorities are prominent among them. Competition among vertical public administrative units is another problem. The centers of clientele-based, mutually-aggrandizing power that epitomize relations between bureaucracy and interest groups are everywhere evident. So mutually exclusive, impermeable and reciprocally accommodating are these centers, it is unlikely that they can be coordinated (for planning purposes) against their will, or persuaded to shift the effective locus of controlled allocations from one sector or level to another. Thus, the reason why interest groups are not very active in the regions or the cities as several authors show, is that they know that effective power to promote or to veto, to accelerate or delay, lies elsewhere.

In these liberal, pluralistic societies of mixed economies, planning is rarely seen as a normative orientation toward what should be allocated to whom, for what purposes, and what processes shall be followed for making and implementing such policies. Planning is seen, rather, as a technical instrument, to be used or not by policy makers as the need arises. Planning becomes merely one of many forms of incrementalism based on mutual partisan adjustment.

As most of these authors view it, planning, as a normative model governing the choice and allocation process, is fundamentally antagonistic toward this type of decision making for public policy. Their case studies show that operating incrementally at the margins, however reasonable or inevitable this approach may appear, tends to obscure longer-term objectives. This is particularly true of redistributive policies, as the French experience seems to attest. At the worst, this conception of planning may create the impression that the institutions of pluralistic democratic societies are working reasonably well, when in fact these institutions find themselves greatly at risk.

Planning concerns power—who holds it and to what ends. This basic consideration often eludes the more zealous advocates of planning, but it is not lost on politicians, especially public administrators. Jobert is therefore correct in lamenting (p. 386) that "structural planning" cannot proceed where administrative bodies remain encased in watertight compartments. Breaking up these compartments is an assault on powerholders. As many essays in this volume show, particularly those on urban planning, the occupants of these compartments have remarkable staying power. Thus, when new administrative arrangements are created to circumvent existing compartments, professionals like the engineers from *Ponts et Chausées* arrive and reassert their hegemony. Or they may come from the treasury, or any of the other prestigious and dominant sectors of traditional administration. Reform-minded economists and sociologists are usually no match for them.

The countries discussed in this book demonstrate how quickly the best planning intentions founder when unaccompanied by single-minded political support. The French Commissariat survives precisely because it has assiduously avoided taking on any of the major traditional feudalities that govern the nation's political economy. French planning seems to have "worked" better than the British and Italian variants because, as Nizard suggests in a short, scathing essay (pp. 433–44), planning has been "the regulatory reproduction of the status quo."

Trevor Smith makes a good case that Britain has been as venturesome as any nation in innovative planning. Guided by the French "model," Britain experimented with new frameworks for encouraging mutual partisan adjustment among business, labor and government. British industry seemed committed to these innovations so long as discourse was dominated by classical assumptions about the centrality of enterprise, and so long as government did not seek to impinge officially on the freedom of the firm. In this setting, the TUC's earlier interest in planning turned sour. As I write, Britain's "social contract" hangs on a thin thread, and the chaos of atomized collective bargaining lurks around the corner. In Italy, once-divided trade unions are universally disenchanted with planning, their leaders acutely aware of their limited control over their members if austerity measures there remain tied to medium-term palliatives designed to stabilize the market.

Advocates of planning, as Trevor Smith says (p. 119), may often be "the prisoners of their own rhetoric." This leads them to be angry when trade union leaders turn a cold shoulder to planning schemes. The dilemma here lies in differences in definitions are expectations over the meaning of planning. As Stephen Young points out (Ch. 5), there are at least five types of planning policies formulated by governments. These range from stabe at comprehensive planning (very rare and never successful) to sectoral planning and the removal of certain obstacles to growth (sometimes successful). There is no persuasive evidence that the decision to plan represented a choice to bring about basic, structural economic and social reforms.

These realities lead Watson to conclude (pp. 477 ff.) that the potential of planning as an instrument for effecting social reform has been largely illusory. That illusion was not dispelled by the advent to political power of social democratic forces in the three countries during the sixties. It may be that Italy's commitment to regional devolution, and similar tendencies in Britain, will bring changes, but it is too early to tell. On the strength of this volume, the prognostications are doubtful. As Watson laconically puts it, planning "cannot effectively challenge the existing balance of forces in society."

What might change this? Little space is accorded this vital question. Watson actually makes an intriguing case that the spread of greater rationality in planning may lead to intensified forms of social disintegration (p. 481). The one step he seems to favor—more participation by those who are the objects of public policies—echoes the demands and expectations of the 1960s. This approach seems ill-suited to the requirement that planning be more self-consciously normative; that planning policies be more long-term and coordinated; that planning have stronger political support; and that a solution be found to overcome the institutionalized opposition to change that one finds in a nation's existing administrative and interest-group configuration. We need blueprints to show how to bring about these radical transformations without destroying what is valuable in liberal democratic societies.

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General Elections in South Africa 1943–1970. By Kenneth A. Heard. (New York: Oxford University Press, 1974. Pp. xx + 269. $18.25.)

It is no mean task to write a history of elections without boring or blinding the reader with psephological science, and it is a measure of Kenneth Heard's mastery of his material that he has succeeded in writing a study which consistently holds the attention. Only very occasionally does one feel that the wood is obscured by the trees. Sometimes also one has the impression that the author has carried too far the self-denial involved in writing so specialized a work, rather than the more general political history which it is to be hoped he will undertake.

Heard devotes a chapter to each of the seven general elections in his period, and one to the referendum of 1960, which chose republican status for South Africa. He shows (following Stultz and Butler) that, although the National Party has been continuously in power since 1948 it was almost certainly not until 1961 that "a majority of the 'projected' national electorate supported the Government" (p. 145), and one cannot but agree with his remark that "rarely can a country have embarked on a new course on the basis of so indecisive an electoral victory, nor one so little expected" though one might question his description of *apartheid* as a *new* policy, and see it rather as the extension and codification of practices which had always been present in the society.

1953 saw the consolidation of nationalist power "for the first time since Hertzog and Smuts had joined forces, the Nationalist political forces stood united in a single political party" (p. 50). Against the NP's simple message of white supremacy the United Front (UP and minor allies) had no simple riposte, though it had many worries about the state of the nation, apartheid, civil liberties and the constitutional question. The result was triumph for the NP though still only 49 percent of the vote. The opposition's major electoral handicap was its concentration in relatively few constituencies, whilst the NP was greatly assisted by the favorable loading of rural constituencies.

Heard entitled his 1958 chapter "The Call of the Tribal Drum." The purpose was to consolidate the unity of Afrikanerdom, and, having won 97 of the 150 seats in 1958, Verwoerd led his people into the 1960 referendum fresh from his narrow escape from assassination. As Die Burger said (quoted on p. 95), "In this miraculous escape all believers will see the hand of God." He had, however, protected himself by enfranchising whites aged 18 to 20, including the whites of South West Africa, and by excluding the colored voters. Indeed, Heard calculates that, had these precautions not been taken, the referendum would have been very close and might well have gone against the NP.

In 1961 no fewer than 67 seats were uncontested; the electorate was becoming partitioned into two blocs of constituencies consisting largely of safe seats, with a very few on the margin, but by 1966 South Africa had moved into a new and disturbing world; Rhodesia had made its UDI, links with Britain and the Commonwealth were slackening, and it was perhaps a sign of a new political unease that no fewer than 345 candidates stood for the recently increased House of 160 seats. The UP focused on race policies and particularly attacked the Bantustan proposals, but the UP's own policies (centering on a rather vaguely described Race Federation) were less clear. What was clear was that they shared with the NP a commitment to white leadership.

In his chapter on the 1970 election Heard gives a fascinating account of the political in-fighting within Afrikanerdom which surrounded the breakaway of Herzog's Herstigte Nasionale Party (RNP). Perhaps, with the clear vision of hindsight, one may conclude that the author somewhat overplays the HNP's importance, though the importance of the issues in contention can hardly be exaggerated. Against *verligte* (enlightened) Afrikanerdom's insistence that all whites must pull together and that South Africa must behave as part of Africa, rather than as an offshoot of Europe, the *verkramptes* of the HNP claimed that their program was based on the infallible word of God, and advocated Afrikaans as the sole official language.

In the event the UP (with 47 seats) won back some of those lost in 1966; the HNP won none (though they scored over 6 percent of the combined NP and HNP poll) and the Progressives still had only one representative in Parliament (the redoubtable Mrs. Suzman). However, the shift of votes to UP and Progressives was significant enough to give Heard cause to hope that what might otherwise appear a "law of ever-increasing rigidity" in South African politics might in fact be an illusion.

For the author does not despise those who hope that South Africa may change, and change meaningfully, without a holocaust. Although some of the most divisive issues (among whites) have been eliminated, and those which remain (the preoccupations with race and security) promote convergence, rather than divisions, of opinion, he believes that "a generational influence may be at work which could presage the advent of a new alignment of political forces, possibly cutting across—at least eventually—the linguistic barrier" (p. 241). Furthermore, he considers that "the emergence of spokesmen of stature in the various racial groups . . . has initiated a process, which can hardly be reversed, of political interaction among those groups" (p. 242) and that by the end of the seventies these new processes will show in new creative responses by the major parties.

For some observers the mildly reformist grouping which has come to prominence since the 1974 election may confirm Heard's modest hopes; for others this development will merely confirm the view that the external appearances of apartheid endlessly adapt to changing conditions, whilst the underlying facts of white power remain the same. For both parties Heard's scholarly study, in some ways the successor to Gwendolen Carter's pioneering *Politics of Inequality*, must be indispensable.

CHRISTOPHER R. HILL

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Elite Structure and Ideology: A Theory with Applications to Norway. By John Higley, G. Lowell Field, and Knut Gröholt. (Oslo and New York: Universitetsforlaget and Columbia University Press, 1976. Pp. xii + 367. $17.50.)

The book by Higley, Field, and Gröholt starts with the premise that there are serious flaws in the existing literature on political elites, and that these flaws in large measure stem from the two major "organizing principles" of studies on political systems. One of these principles "assumes politics to be a reflection and consequence of more fundamental societal features"; the other "sees politics as considerably independent of such phenomena" (p. 2). While the first principle assumes very strict parameters for elite activity, the second posits very wide latitudes for those who occupy elite positions in political systems. Having duly criticized this state of affairs, the authors go on to emphasize the interaction between society and polity, thus attempting to account for the social, economic, and cultural parameters of political acts, while at the same time stressing the voluntaristic aspects of decision making at the top in any political system.

After the discussion of societal-political interaction, the authors go on to examine political stability and change, and find that a decisive variable in this context is "elite unity" or "elite disunity." The definition of a unified elite is "one sufficiently cohesive and freely communicative internally so that regardless of the different political affiliations which its members may maintain and the different interest positions in the society with which they may associate themselves as spokesmen, they share a basic consensus about the nature and the desirability of the society they direct and manage" (p. 20). A disunited elite lacks these characteristics, and the unified elite, then, is essentially committed to continuing the status quo.

Under what circumstances does a unified elite develop? The book discusses the historical developments which tend to produce unified elites (ideologically or consensually unified) in some detail, drawing on the experience of (among others) the Soviet Union, China (ideologically unified elites), and Great Britain, Sweden, Mexico, and the United States (consensually unified). Furthermore, the book attempts to relate types of political elites to socioeconomic development. In this attempt, four levels of socioeconomic development are discussed (level one, the simplest agrarian societies; level four, societies where more than half of the work force is made up of nonmanual occupations; levels two and three are intermediate stages). From this basis the authors argue that in developed societies, elites (a) are recruited because of formal qualifications, (b) tend to perpetuate this situation by recruiting the same kind of people to succeed them, (c) relate to other elites in special ways based on elite status per se rather than on kinship or personal acquaintance, (d) become essentially managers of shared symbols and values—a habit which tends to unite the elite in a common conceptual framework, producing greater social distance between elites and the rank and file of the population and thus greater stability and less capacity for change. Such unified elites, then, are said to be in existence in countries which have reached levels three or four of socioeconomic development. Such an elite would therefore be better able to handle the diverse and complex social, economic, and cultural needs of highly developed societies.

After the theoretical introduction, the book goes on to discuss the case of elites in Norway, which is seen as belonging in the transitional stage between levels three and four of socioeconomic development. The main focus is on four elites: the business elite, the labor union elite, the civil service elite, and the political elite (which, in short, is made up of the top professional politicians in the government, the party hierarchies, and the parliamentary leaderships). A great deal of information is gathered on elite backgrounds and careers, elite interaction, and elite ideology. The concluding chapters deal with the extent to which the Norwegian elites have remained consensual; a final chapter discusses structural changes in the societies of higher development.

Not surprisingly, the authors find a high degree of consensus in a wide variety of fields and issue areas among the four types of Norwegian elite examined. The meticulous tabulations and cross-tabulations thus tend to support other findings of Norway as a stable democracy. Despite this, the general conclusion is not all roses. Specifically, Norwegian elites seem lacking in perspectives for the future—especially on the conflict between those who have the skills required in highly developed societies (the "insiders") and those who do not ("the outsiders") and who therefore will be doomed to a marginal existence on the fringes of society, thus constituting a likely source of political and social upheaval.

In many ways, this is a good and important book. The theoretical chapters are well written, and the field study is kept well within the bounds of the general hypotheses. I would nevertheless suggest that the theoretical framework is not as innovative as implied by the authors; many, perhaps most, students of political elites and their behavior have been aware of the relationship between societal parameters and elite autonomy, and quite a few have done good work on it. It is furthermore a weakness that the theoretical framework does not seem to hypothesize well the widespread *instability* of highly developed societies which we have seen during the 1970s. True, the last chapters partly remedy this, and the authors labor mightily with this problem, emphasizing that this instability is increasingly to be expected because of the "outsider" element in such societies; but the framework did not adequately draw attention to this phenomenon. I also had some quarrels with the Norwegian case study, specifically in the area of elite selection (why was there so little emphasis on the "brain trusts" of intellectuals, for example, who have been researched in other highly developed societies and found to be of considerable importance as providers of specialized information?). Furthermore, there were some cases in which only a part of the elites examined had actually been interviewed, without a detailed explanation of *why* and *how* the selection had been made (this apart from the actual definition of positions and individuals belonging to each elite, which I found perfectly adequate for the Norwegian case). Finally, I had some reservations about some of the conclusions drawn from the data, especially those conclusions pertaining to the labor union and Labour party elite, who may have been moving a bit further to the left (according to some analysts), rather than closer to the other elites, especially in the 1960s. Finally, the book needs updating. The events of the early 1970s, in which large numbers of rank-and-file party members crossed political boundaries to vote for or against Norwegian membership in the Common Market may have resulted in fundamental political realignments in Norway. What, if any, have been the ramifications of this development for Norwegian elites? The authors deal with this to some extent in the last few chapters, but the bulk of the data covers the 1960s. An update for the 1970s would make the volume even more valuable.

Despite the reservations listed above, I find this to be a good book well worth reading, both for those interested in theoretical considerations on political elites, political stability, and development, and for those who have a more specialized interest in Norwegian politics. The book joins the list of systematic and sophisticated research on Norway and is a worthy addition to that list.

The footnoting is adequate; there is a good bibliography, but unfortunately, no index.

TROND GILBERG

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Women in the World: A Comparative Study. Edited by Lynne B. Iglitzin and Ruth Ross. Studies in Comparative Politics, no. 6. (Santa Barbara, Calif.: American Bibliographical Center, Clio Press, 1976. Pp. xviii + 427. $19.75, cloth. $6.50, paper.)

This volume stems from a 1974 symposium sponsored by the Center for the Study of Democratic Institutions and the University of California at Santa Barbara. The editors describe the focus of the book as "sex roles and male-female relationships as they affect and are affected by the political system..." (p. xvi). The papers are organized into four sections: Conceptualizing the Cross-Cultural Study of Women, Women in Europe and the United States, Women in Developing Countries, and Women in Nations Mobilized for Social Change. Lynne B. Iglitzin presents a model for classifying societies according to their degree of "patriarchy." She sees patriarchy as a set of five attitude clusters that have provided the rationale for the subordination of women: (1) sexual division of labor reflects natural differences; (2) women's identity comes through their relationship with men; (3) women get greatest fulfillment as wives and mothers; (4) women are childlike; and (5) women are apolitical (p. 15). Essays by Jaquette and Van Allen make appropriate warnings about the dangers of imposing Western developmental conceptualizations on other cultures and propose some interesting variants on the generally accepted measures of participation. These three pieces offer some initial conceptual guidelines for the volume, though no such framework was imposed on the contributors.

The subsequent pieces, organized broadly by type of socioeconomic system, give descriptive case studies of the political, social, or economic status of women in different societies. The overall approach of the volume is cross-cultural, not comparative in a formal sense. An exception is the piece by Blumberg that presents a pilot comparative ethnographic study to test hypotheses about women's economic power, though she also utilizes a case study of changing roles of Israeli kibbutz women. Generalizations from the data pieces are hampered by a lack of explicitness about conceptual and measurement criteria. While describing the political and economic status of women, few of the contributors come to grips with their criteria for equality (e.g., opportunity, condition, sameness, corporatism, etc.). The field of women's studies is not ready for consensus, but explicit criteria would help to clarify the discrete pieces. One of the strengths of the case studies is the richness that comes from the individual perspectives, but it is also a drawback when trying to use the reports as a data base for comparative analysis.

The predictable finding of most of these studies is that women do not share equally with men in the economic and political "goodies," though there are certainly differences in degree of subordination across time and culture. Of more interest is the variety of implicit and explicit explanations of the observations. The role of religion, as outlined in Porter and Venning's comparison of the role of the Catholic Church in Italy and Ireland, is an example of this explanatory analysis.

This volume is one of the first efforts to present cross-national research on the status of women, but the test of the approach will come with subsequent, systematic comparative research. The structural relationships that may covary with the socialization practices are outlined in the cross-national approach so that many of the pieces of that puzzle, for example, are available. The connections between government proclamations, political ideology, economic development, religious values, and sexual equality are not developed; the process and pace of change needs careful analysis. One might infer from Iglitzin's patriarchal model that female-male relationships form the substructure of the social system. Although many of the articles suggest other alternatives to such a "sexual politics" approach, the cumulative effect of these essays is to suggest that sex roles may operate as an independent function of the social system rather than as the byproduct of of particular sets of economic, political, or cultural relationships. While one can easily fall into the chicken-and-egg trap, setting up some of the causal relationships for systematic testing will be an important next step.

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Idols of the Tribe: Group Identity and Political Change. By Harold R. Isaacs. (New York: Harper and Row, 1975. Pp. 242. $10.50.)

It is one of the odd paradoxes of our time that, for reasons not wholly understood, much of the world has seen ethnicity assert itself as a force emphasizing the deep inner fragmentation of peoples in what is generally acknowledged to be an age of bigness and of global interdependence. As Harold Isaacs puts it in the opening chapter which sets the stage for the remainder of his book, we are experiencing "not the shaping of new coherences, but the world breaking into bits and pieces, bursting like big and little stars from exploding galaxies. . . . So we are refragmenting and retribalizing ourselves" (pp. 11, 24).

Idols of the Tribe undertakes to explore the nature of basic group identity, to use Isaacs' key term, and the major elements composing it, with a constant eye out for the effects of political change. Isaacs is in a unique position to fulfill this assignment because of the many years of fruitful labor which he has invested in an impressive succession of inquiries into the structure of different ethnic groups and their views of themselves and of their neighbors, friends, and enemies. His primary research technique is to conduct intensive interviews with members of a group and other people concerned. In contrast to his previous research and writing which focused on a particular people or pair of peoples, *Idols of the Tribe* approaches mankind at large as its subject, seeking to establish the fundamental principles to be derived from his earlier researches and to interpret the effect of changing power relationships. Isaacs' invaluable storehouse of materials gathered in the past illustrates and illuminates his argument.

After two introductory chapters, one of which deals with the psychological approach and particularly with the contribution of Erik Erikson, Isaacs turns to his main theme of examining the elements which make up basic group identity. He proposes to begin at the beginning, and takes as his starting point the birth of a baby; however he is not concerned to establish that something new has been added to a group but that the new-born infant carries with it a vast heritage from the past. One is the inheritance of the shared physical characteristics of the group—skin color, features, texture and perhaps color of hair—which serve as the most obvious and permanent badges of identity. Also the baby is predestined to be exposed as a part of its cultural heritage to the language and ways of life, the religion, the myths and history of the group. This heritage serves to link the past to the present and the future. For most people most of the time basic group identity and the elements composing it are relatively fixed and durable but Isaacs rightly insists that they are not immutable and static entities but dynamic and subject to change: "We are now going through a time of great and heaving change in all the conditions that affect every person's life and circumstances" (p. 41).

The chapter on nationality left this reader confused as to what Isaacs regards as basic group identity and how he distinguishes it from nationality. As he moves from the preceding chapter on religion, to nationality, which he takes to be only a step away, he declares that we seem finally to come face to face with basic group identity "which comes into view most often dressed in its national colors, marching under its national flag, wearing its national tag" (p. 171). An arguable case can be made for the proposition that the nation or nationality is the ultimate, most rounded and inclusive form of basic group identity; but something seems out of kilter when basic group identity appears *most often* in its national colors, flying its national flag. Most of the states of the world cannot legitimately be called *nation*-states, nor can most of the increasing number of ethnic groups, which have of late attracted so much attention, be seen as nations or nationalities, but rather as aspirant entities which resist the melting pot of the state of which they are a part.

This book again demonstrates Isaacs' ability to break away from conventional academic jargon and style, and to capture his readers by flashes of wit and provocative turns of phrase and ideas as well as by the acuteness and solidity of his material and presentation.

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Rebels and Bureaucrats: China's December 9ers. By John Israel and Donald W. Klein. (Berkeley and Los Angeles: University of California Press, 1976. Pp. xiv + 305. $15.00.)

This volume is a felicitous example of scholarly collaboration: a successful merger, in this instance, of two disparate research talents. Each of the authors comes to this project with impressive credentials in Chinese studies. Whereas Israel has received recognition for his writings on the history of student politics in China since the 1920s, Klein has been the foremost authority on biographical studies of Chinese Communist leaders. In the authors' joint venture, China's "December 9ers" are subjected to analysis that is essentially historical biography (or more accurately, perhaps, prosopography, as the authors suggest, p. 8).

With such an analytic framework an important elite segment from modern Chinese history is fruitfully examined as a "generation," as the intersection of a particular age group with historical time.

From a structural standpoint, a division of labor in the writing of this work seems apparent insofar as sections of the book tend to concentrate either upon aggregate elite analysis or upon the historical interpretation of lives and events. Yet the mix of specialties is not stylistically obtrusive, and the book remains, in general, a coherent and integrated whole.

Thematically, *Rebels and Bureaucrats* should be of special interest to social scientists who focus on comparative problems of students and politics and, to a lesser extent, those who engage in comparative studies of elites or of revolutions. A caveat should be appended, however, to the above recommendations. Israel and Klein's findings contain a considerable amount of detail, especially of a biographical nature, such that scholars who are not China specialists may be put off, particularly at encountering so many Chinese proper names.

As a case study of students and politics, *Rebels and Bureaucrats* makes a valuable contribution to the comparative literature on this subject. The overarching motif of student political action in this period of Chinese history, the mid-1930s, was nationalism, with a concomitant anti-imperialism. The advocacy of nationalistic aspirations has been documented for student movements in a number of other Third World nations.

Social scientists have often commented upon the evanescence of student activism, a phenomenon attributable in part to the structure of educational systems and, simply, to the passage of time. The influence of China's December 9 movement has been less fleeting than that of many other national student movements. Its peculiarity in this regard seems to stem from the timing of concrete historical events: the rapid onset of war against an external enemy, followed by massive civil conflict, and then the establishment by the Communists of a new regime, the People's Republic of China. These events—singly and, especially, in succession—increasingly opened up opportunities for the youthful activists. In the closing chapters of *Rebels and Bureaucrats*, however, the story takes a familiar and perhaps inevitable turn with the metamorphosis of once strident student demonstrators into stolid middle-aged bureaucrats, members of the establishment.

The December 9 material chronicles another finding from comparative politics: the uneasy alliance between radical-leftist political parties and young intelligentsia. In the example of the December 9ers, this relationship was further strained by the strong streak of populism in Chinese Communist theory and practice. But systemic exigencies favoring Communist party reliance upon the young urban intellectuals seem to have counteracted centripetal ideological forces that questioned their ultimate loyalties. The talents of the December 9ers were indispensable during the nascent years of the Communist government, when a serious shortage of experienced technical personnel existed. By contrast, many developing countries (including China in more recent years) have been plagued by a surplus of educated youth, not readily absorbed into positions commensurate with their abilities and training.

In comparison with prototypical studies of elites, this work on China's December 9ers demonstrates some considerable strengths along with a few minor weaknesses. A fairly common shortcoming of elite research conducted by political scientists is the ahistorical character of their analyses. In this portrayal of the December 9 generation in China, however, elite biographies are embedded within—not divorced from—the broader historical context. For example, the Sino-Japanese conflict, Kuomintang-Communist rivalries, and rural-urban estrangement are all essential backdrops to the fortunes of this group of students. A second, and related, attribute of the Israel-Klein work to be commended is its inherent dynamism. Rather than presenting a static view of their subjects at one or possibly two points in time, the authors have, within the limits of the data, carried out an effective longitudinal study extending over several decades. Thirdly, this volume goes beyond the mere establishment of commonalities in elite backgrounds, careers, and so forth to an assessment of the social consequences of having an elite with such characteristics and experiences. Too often elite studies stop at the discovery of elite configurations, failing to go on to probe the effects of such patterns.

The authors do fall prey somewhat to a not uncommon fault of inflating the importance of one's subjects. The December 9ers were not, after all, a large student group when they demonstrated in 1935 (estimated at 2,000), nor did they come to constitute *the* elite in New China after 1949. Rather, we may justifiably conclude that this small generational subgroup of student activists, who evolve into a portion of the national elite, has had a genuine—but limited—impact on the Chinese political scene during the transformation from revolution to regime.

In their portrayal of the December 9ers, Israel and Klein are rather sketchy when noting differences *within* this generation, emphasizing, instead, its homogeneity. With conceptual devices such as generation (or cohort), class, etc., there is the underlying danger of reification of a social category. Admittedly, however, the December 9ers, as a social entity, have exhibited a significant degree of self-consciousness, an attitude conveniently nurtured on occasion by the leadership of the Chinese Communist party. More explicit attention might also have been drawn to specific differences *between* the December 9ers and other prominent elite aggregates. The uniqueness of this generation tends to be assumed and the reasons for its atypicality glossed over.

The December 9 narrative covers a broad sweep of social change. Indeed, a classical transformation from revolution to routinization comes alive through the collective biographies of this generation of leaders, as the Young Turks eventually became the Old Guard. Maoist insistence on continuing revolution for China meant in this instance, however, a premature finale during the Cultural Revolution to many promising bureaucratic careers. In conclusion, Israel and Klein's *Rebels and Bureaucrats* is a rich comparative resource; their work provides much carefully sifted data for further analyses of revolutionary change, of elites or of student politics.

AMY AUERBACHER WILSON

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Military Conflict: Essays in the Institutional Analysis of War and Peace. By Morris Janowitz. (Beverly Hills, Calif.: Sage Publications, 1975. Pp. 319. $16.50, cloth; $6.95, paper.)

Morris Janowitz is no longer a one-man marching band writing sociological studies on the military. But he is still the fearless leader of the troop and, to vary the metaphor, an indefatigable academic enterpreneur whose achievements include founding the Inter-University Seminar on Armed Forces and Society, a counterpart research committee of the International Sociological Association, and a learned journal, *Armed Forces and Society*.

In this book Janowitz reprints a dozen essays written since 1948. Some deal with the American military, others with Western countries generally, a few with new nations and the international arena. Since all examine the strong arm of the state, it is understandable that Janowitz should draw frequently, if somewhat selectively, on political science literature. But as a sociologist, his consistent aim is to show how his own discipline can illuminate changes inside the armed forces and in their relationship to society at large. In explaining morale in Nazi armies, for example, Janowitz emphasizes the pivotal role of primary group membership, much as Elton Mayo and Fritz Roethlisberger did in their earlier research on the morale of industrial workers.

Several essays deal with civil-military relations in comparative perspective. Janowitz's search for the *causes* of military intervention leads him to distinguish "designed" from "unanticipated" militarism just as economists distinguish "cost push" from "demand pull" inflation. His inquiry into the *effects* of intervention—on reform, consent, economic growth and political stability—ends by acknowledging the difficulty of the enterprise and by suggesting that researchers examine small clusters of similar nations instead of attempting worldwide generalizations. In one of the more empirical articles, Janowitz takes his own advice by concentrating on Middle Eastern armies, especially on Attaturk's achievement in removing the military from Turkish politics.

Two essays touch on international politics. One is essentially a plea for sociological studies of arms control. The other argues that military "systems" (policies?) should be evaluated as "stabilizing" or "destabilizing" in terms of their effects on international rivalry. "Destabilizing" phenomena include improved missile accuracy, civil defense programs, nuclear proliferation, and American ground forces in Vietnam. "Stabilizing" phenomena are invulnerable deterrents, test ban treaties, hot lines, and American ground forces in NATO. This is accompanied by an argument, not exactly new, that strategic theorists of the last generation often spun out their deadly logic in a political vacuum.

Janowitz's more important contributions come in tracing the rise and decline of mass armies. Starting with a major essay entitled "Military Elites and the Study of War" (1967), he contends that advances in military technology created a demand for trained specialists and, therefore, for a more democratic and representative military; that conscription had important egalitarian implications for citizenship; that large citizen armies grew increasingly to resemble civilian organizations; that managerial prowess came to rival heroism as military roles expanded; and that officers began to rely on manipulation and not merely on domination as a basis of authority.

But Janowitz can rightly claim also to have foreseen many of the counterforces now limiting the convergence of civilian and military institutions, and which may once again isolate the latter, especially the ground combat forces, from society. The improbability of major war, the emphasis on professional forces in being, the replacement of conscript by volunteer armies, the erosion of nationalism, the lack of interest on the part of middle-class whites in military careers, the over-representation of blacks and blue-collar workers, the inbreeding of an officer corps drawn increasingly from service families—here are the sources of the present crisis of the military profession. How can soldiers retain a sharp edge and a healthy self-image when confined to training exercises? How can the military preserve its legitimacy when it is not only unused but isolated and unrepresentative, an island of conservative nationalism in a boundless liberal sea?

Though cautious about projecting current trends, Janowitz is quite ready to recommend policy changes. The armed forces should be used for peacekeeping functions, for environmental activities, and for coping with national disasters. Morale should be maintained by curtailing rotation and by encouraging personnel to identify with particular regiments. Parochialism should be countered by reducing reliance on in-service education at the pre-commissioning and war college levels. The past decade, Janowitz suggests, was the worst possible time to cut ROTC while simultaneously expanding the service academies. Amen.

There is much sustenance here for the reader who can stay the course. Just two small sour notes in closing. Janowitz's professional reputation is not likely to rest on his literary style; and, like seasoned veterans on other campaign trails, he does not shrink from repetition.

LAURENCE RADWAY

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The Shrinking Political Arena: Participation and Ethnicity in African Politics, with a Case Study of Uganda. By Nelson Kasfir. (Berkeley: University of California Press, 1976. Pp. xvi + 323. $23.50, cloth; $11.00, paper.)

Nelson Kasfir's *The Shrinking Political Arena* is a first-rate book which tackles one of the most central and pervasive problems of African ethnic political struggle. Although Kasfir draws his examples exclusively from black Africa and makes Uganda his key case for detailed study, his general theoretical analysis will interest any student of comparative politics.

Kasfir's essential analysis helps explain the movement away from participatory institutions in post-independence African states. In his view "departicipation" is the most common antidote to rising demands based on ethnic identities. However, it has the potential to be both a positive adjustment and an explosive response to fissiparous tendencies. The book begins with an elaboration of general propositions about the complex and contextually varied character of political development, participation and ethnic identity. As a former student of Samuel Huntington, Kasfir builds his analysis upon the general framework of Huntington's *Political Order in Changing Societies.* However, Kasfir challenges the assumption that societies move ineluctably from lower to higher levels of political participation. Decay of public institutions may be prevented, he argues, both by adroit response to pressure and by consciously promoting lowered participation. Although participation rose dramatically during the period of independence movements in African states, it can be and has been reduced in the post-independence period by a variety of techniques.

Kasfir offers two types of case analyses to support his propositions. First, he reviews six cases in Uganda where ethnic-based demands created problems for government. Second, he compared the Uganda situation with other countries, most frequently Tanzania. In some of the Ugandan cases the salience and disturbance of ethnic demands was reduced by giving in to them, for instance by letting the "lost counties" of the expanded Buganda kingdom return to Bunyoro and giving the Sebei a separate district. Indeed, if bases for mobilizing ethnic claims are recognized before their potential is realized, the expansion of ethnic participation may be alleviated by early action, as in the creation of a separate Madi district.

Of course, in other instances rivalry among ethnically organized claims may be incompatible, as seems to be the case with regard to the Buganda (16 percent of the population) in their frequent antagonisms with other groups within Uganda. Because the Buganda are overrepresented among business and government elites, and were favored by British colonial policy, they are resented by other ethnic groups. And the Buganda, in turn, have felt ethnic deprivation, especially under Obote. Such feelings can be easily mobilized. General Amin, by his release of Buganda detainees following his coup in January, 1971, enjoyed dramatic initial popularity among the Buganda, for instance.

Kasfir indicates how intractable rivalries destabilize politics and require either delicate ethnic balancing to minimize their effects (though not reducing the political salience of ethnicity) or long-term efforts to dampen ethnic awareness by practices such as excluding accounting of it from census and business forms and creating electoral constituency rules that cut across ethnic boundaries. Unfortunately, an elaborate electoral plan to accomplish this was aborted by the Amin coup. Under General Amin ethnic identities have been given more explicit recognition, but apparently with mixed effects upon the mobilization of ethnic demands.

Kasfir reviews how many African states have experienced difficulties similar to Uganda's. Political participation stimulated by ethnic assertions has resulted in coups or repressive government actions in many states. Such participation, however, is not uniform, but complex in origin and reflects modernizing pressures rather than traditional or unchanging primordial identities. Tanzania, Kasfir finds, may be a unique case where the government succeeded in limiting participation without stimulating excessive ethnic resentment or promoting military intervention. Certainly the absence of one or a few large ethnic groups has helped Tanzanian policy to succeed. In addition, Kasfir believes the broad base of support which TANU achieved initially, combined with the slow, balanced steps taken to shrink the arena in which political disputes might be aired, also account for the successful Tanzanian pattern of reducing ethnic-based participation.

The book suffers from two weaknesses. First, Kasfir presents rather scanty precise information on aggregate ethnic arithmetic, e.g., geographic distribution of tribes or extent of over-representation in elite positions such as the government or military. His case studies would be considerably strengthened by information on such important distributions and their changing composition. Unfortunately, his most recent data is from 1969. The second weakness is that the book is best appreciated by an audience knowledgeable about Africa or Uganda. While novices can benefit from the book, and the bibliography is useful, many of Kasfir's arguments cannot be fully comprehended except by someone already familiar with Uganda's political history and African politics in general.

Kasfir's volume represents an important addition to the second generation of scholarly books on Africa. Its conclusions, which return to the level of broad generalization with which the book begins, are thoughtful and balanced. During the first generation of African scholarship among Americans, most works were country studies also illustrating ideas about topics such as nationalism or institutional transfer. In the 1970s such works are necessarily being succeeded by books such as those by Kasfir, Bates, Sandbrook and DeCalo that have a broader scope of cases and/or greater theoretical application to post-independence politics.

RAYMOND F. HOPKINS

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Resistance and Revolution in China: The Communists and the United Front. By Tetsuya Kataoka. (Berkeley: University of California Press, 1974. Pp. 394. $16.50.)

As the Chinese Communists enter their 27th year of rule over their vast country, scholarly search for a more satisfactory answer for their victory in 1949 continues. The key questions remain: was the 1949 Communist victory inevitable? And if so, was it the Maoist strategy of peasant revolution that made the victory inevitable? Volumes of research have been published to shed light on these questions, and thus far the predominant views tend to be positive. In spite of this, there seems to be a revival of academic interest to search deeper, in an attempt to acquire more detailed information for the critical period of 1937–1949. Some have focused their inquiry on what went wrong with the Chinese Nationalists, the Communists' adversaries, whereas others expound the Communist strategies and activities during those years. Tetsuya Kataoka's current volume pursues the latter course.

The book actually centers on the years from 1937 to 1943, during which time the Communists and the Nationalists entered a second united front against Japanese aggression. In 1943, two years before the war ended, the united front was virtually broken down when the Communists were ready for an armed insurrection against the Nationalist Government. Kataoka appears very cautious in dealing with the two questions posed above. In fact, he never tries to answer directly the question whether the Communists were indeed mandated to rule in 1949. Perhaps this is because his research stops with the year 1943, and unlike many courageous souls, he chose not to speculate beyond what his topic and materials permit. I salute his caution as a careful researcher and scholar. His answer to the second question is direct, at least insofar as it applies to the 1937–1943 period. The author suggests quite convincingly that Maoist peasant strategy alone did not account for the rapid expansion of the Communist strength during those years. In fact, without the war with Japan, a united front with the Nationalists and a skillful exploitation of emerging urban nationalism, the rural strategy alone could not have accomplished what the Communists did.

Such a view, to be fair, is not original with the author. His contributions in this case, lie rather in his articulation of such a view which he expounds with a wealth of information that makes it convincing. In military terms, the Communists were thoroughly beaten by the Nationalists by 1936. However, the subsequent Nationlist-Communist united front in 1939 diverted the Nationalists' strength to resist the invading Japanese. Japan's occupation of cities in North and East China and the Nationalists' fleeing to Szechwan created a vacuum of organized political power in the vast rural areas of North China. It was in those areas that the Communists succeeded in establishing bases by absorbing the existing rural forces such as the bandits and the secret societies. Once the Communists did gain control over rural areas, land reforms were conducted to liquidate the landlord class and to mobilize the peasants. Thus resistance against Japan and an awakening nationalism against foreign invasion were skillfully exploited to strengthen the Maoist strategy of rural revolution.

Professor Kataoka's book is particularly strong in dealing with the strategic differences between Mao and Wang Ming. Nowhere in existing works can we find such a thoroughly documented treatment of their differences, namely between Mao's rural strategy and Wang Ming's persistent emphasis on urban revolution. The author's familiarity with Japanese language and policy measures during the war time fills an important research gap that is often beyond the reach of China scholars. By carefully using the documents and materials held by the Nationalist authorities as well as the Japanese, the book provides by far the best and most detailed description of the Communist land programs in North China's rural villages during the war.

In summary, this is a thoroughly researched volume. No sweeping theory is contemplated, but useful insights and keen observations are plentiful. One only wishes that the book had told us more about the Communists' party organizational activities (recruitment and training of members, structural network, etc.), mass organization of students and other non-peasant population groups, and the party's activities in urban areas. These are shortcomings for an otherwise excellent volume. China specialists as well as social scientists interested in revolution and socio-political changes should benefit enormously from a careful reading of this book.

HUNG-MAO TIEN

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Lessons from Japanese Development: An Analytical Economic History. By Allen C. Kelley and Jeffrey G. Williamson. (Chicago and London: University of Chicago Press, 1974. Pp. xvi + 285. $15.00.)

This book is an analysis of that difficult period, the Meiji era, in the fascinating story of Japanese economic development. This period, 1868–1912, is analytically difficult because of the unreliability of data and the difficulty of defining and classifying economic variables and institutions. In fact, Kazushi Ohkawa and Henry Rosovsky in their recent *Japanese Economic Growth* (Stanford, 1974) exclude the nineteenth century from their formal analysis on the grounds that statistical data are unreliable. Despite the popular acceptance of the *Estimates of Long-Term Statistics of Japan Since 1868* (Tokyo: Toyo Keizai Shimposha, 1965- ; 4 volumes of 13 remain unpublished), these and other specialists are aware that serious questions about their reliability remain unresolved. Kelley and Williamson also defer to these statistical weaknesses. They begin their analysis in 1887 instead of two decades earlier and extend the time by three years to 1915; they exclude the service sector because the data and classifications there are particularly uncertain, and they assume a closed economy. Notwithstanding, these two perspicacious students of Japanese economic history make a persuasive historical case for their highly abstract two-sector model. However, the authors frankly and probably accurately state: "Competing historical models are likely to produce future Meiji economic histories in conflict with ours" (p. 197), though most conflicts will be minor in nature.

A distinguishing feature of their model is the neoclassical assumption that wage rates are determined by the marginal product of labor instead of being determined institutionally. This assumption is supported by other evidence from the earlier Tokugawa Period that strongly suggests the presence of marginal calculation. These include nationwide household decisions on population control involving family size and sex composition. In this category may also be placed decisions on the amount of nonagricultural production to be undertaken by the farm household and on whether family members would work part of the year off the farm in a nearby or distant city. Similarly suggestive is the existence of a national market with myriad commodities and services produced and purchased by the people at large.

Two most important objectives of the book are (1) to employ sophisticated theoretical and quantitative techniques to write a "new economic history" of Japan and (2) "to confront several of the leading development issues which appear particularly relevant to today's developing nations" (p. 4). The two objectives are partially accomplished by the extensive use of counterfactual analysis; that is, by asking the model what would have happened if a particular condition had been different. For example: they ask for the change in the growth rate of per capita income, if Meiji Japan's population had grown at a three percent rate, a rate typical of the LDC's of today, but about three times the Meiji rate. They conclude that change in per capita income would have been negligible (pp. 21–22, 128–37). One may inquire: will the three percent population growth rate of today's LDC's also prove to have an insighificant effect on their national income growth rates? This is highly problematical. The fact is that Japan underwent a period of retarded population growth in the Tokugawa Period during which per capita income had risen and social institutions had developed that made the Japanese essentially modern by the beginning of the Meiji era. Most LDC's have not experienced a comparable period (and may not need to if some present population and economic growth trends hold true).

One of the most useful services performed by the authors is to survey the literature on Japanese economic development as part of a "systematic review of a wide range of potential factors accounting for ... early Japanese growth" (p. 19). In the process, they help to discredit what has come to be known as the Japanese model of economic growth which was predicated on rapid growth of agricultural production as the source of the surplus necessary for growth. They declare, "We have come to refer to Japanese growth performance as 'miraculous' and to the Japanese 'model' of development for so long that it may come as a surprise to some readers just how ordinary the Meiji aggregate growth experience was. . . . The GNP per capita and per worker growth rates are roughly comparable to the long-term rates achieved by the North American and Western European nations" (pp. 95, 97). The authors also raise important questions on many other issues of development theory and Japanese economic growth and often offer their own conclusions and conjectures. Because this provocative and well-written book challenges or modifies a number of generally accepted views, it will stimulate much new research, a mark of a major work.

JAMES I. NAKAMURA

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Political Change in the Philippines: Studies of Local Politics Preceding Martial Law. Edited by Benedict J. Kerkvleit. (Honolulu: University of Hawaii Press, 1974. Pp. ix + 261. $6.00, paper.)

The editor of this volume posits that through the presentation of political change case studies at the local government level "a more accurate understanding of national political trends" is made possible. He presents the readers with four case studies, one on generational political changes in a village, two on provincial level political changes and the fourth "analyzes political changes among chartered cities." The essays, collected from two conferences sponsored by the Association of Asian Studies in March and November 1972 are summarized by David Rosenberg in a concluding essay.

The first essay by the editor, Ben Kerkvleit, is perhaps the best researched and most suggestive. It is about a village (barrio) in Nueva Ecija, Central Luzon, Philippines. The focus is on what policy and elite changes occurred as the result of the post-World War II Huk rebellion in that region. In actuality, the essay offers more than is promised by the author. It provides the reader with an insightful case study of the breakdown of traditional institutions, and of the political mobilization of peasants caught in the processes of socioeconomic change. Focusing on the socioeconomic structure, the author carefully lays the base for understanding socioeconomic links to political changes. The patron-client system and two types of land tenure structures share tenancy *(kasama)* and fixed-rent tenancy *(buwisan)*, are described. The basic sources of change, the author argues, have been the evolution of elite norms (from traditional "feudal" to modern efficiency and profit); modernization (changes in the economic system and technology); and increases in the population/land ratio. These changes contribute to the landlords' failure to live up to expectations set by the norms of the patron-client based socioeconomic system, and ultimately to the peasant rebellion. Thus the Marxian thesis of mobilization joins those of Deutsch, Huntington, et al. who argue that political change occurs as the result of modernizing forces. Here modernizing factors, for example, changes in economic/organizational mechanization and the availability of scientific inputs) prompted *later* changes in the socioeconomic relationships. Expansions of cash economy, the demand for cash crops, and increased reliance on the central government are also suggested to be among the reasons for socioeconomic and political changes in the village. While the rebellion did contribute to land reform legislation, the actual impact of this policy change was less than desirable for the peasants. Rather than changing tenants into self-reliant landlords, it prompted increases in the *buwisan* tenancy system and further accentuated the gap between the rich and poor. The key finding is that mobilization-frustration, followed by repression lead to peasant passivity rather than to the expected changes. Among the strengths of the article are numerous insights into the change process; for example, the majority of peasants in Central Luzon were tenant farmers whose principal desire was to reform, not abolish the tenancy system. . . ." This socialization partially explains the peasant's satisfaction with minimal pay-offs, even symbolic ones. Among factors not explored is the evolution of norms and their important impact—for example, the evolution from the traditional concern for *pakakisama,* or smooth personal relations, to efficiency (by the second generation landlord) and equity (the peasant's emerging expectation) nor does it discuss the inherent tension between these values.

The second article, "Changing Patterns of Leadership Recruitment and the Emergence of the Professional Politician in Philippine Local Politics," by K. G. Machado, focuses on the impact of socioeconomic changes on local political elite changes. Studying political elite changes in the Visayan town of Capiz and the Southern Luzon town, Batangas, the author carefully describes sources of elite change (e.g., family background, residence, and education). The author concludes that the mayors were becoming increasingly representative in terms of their socioeconomic background prior to martial law. It can be implied from this description of the trend from traditional families (landed elite) to the new "upwardly mobile" political elite that this was primarily due to deterministic factors, in particular socioeconomic changes. A careful examination of alternative hypotheses such as those mentioned by the author (pp. 120–21) would be useful.

The third piece, "Changing Political Alliance Patterns in Rural Philippines," by Louis Benson, is a case study set in Camarines Norte in Southern Luzon. The focus is on the *liders* or politicians who serve "as a link between national leaders and the local community." This article is perhaps the weakest of the set. Its forte is a description of *liders'* views and relationships; however, it is atheoretical and does not explore or conclude anything about the changing origins of leaders. There are hints of evolutionary factors of change (as opposed to those which occurred by Marcos' assumption of martial law powers in 1972), but these are not developed. Thus the author observes that "as the number of clients for a patron increases, the ability of the patron to provide for his clients diminishes" (p. 142).

Perhaps the most rigorous, certainly the most methodologically sophisticated article is Thomas Nowack's and Kay Snyder's on "Economic Concentration and Political Change." This study is guided by the hypothesis that "local machine strength decreases as general sociodemographic processes such as social mobilization and urbanization result in specialized cliental forms of exchange; and further that local machine strength decreases as intraelite competition intensifies." The chartered city is their main unit of analysis. The authors discuss the effects of capital accumulation by indigenous economic elite and foreign investors on local political systems. They conclude that, in both cases, capital accumulations are dysfunctional to development and political stability: "Deterioration in the integrative ability of political machines seems to be both a cause and consequence of conditions increasing the concentration of income among the top 20 percent . . . of the population" (p. 228).

Perhaps one of the basic weaknesses in the otherwise strong case studies is their lack of theoretical concern. This is most seriously missed in the concluding section by David Rosenberg. The book primarily focuses (implicitly) on socioeconomic determinants of political change. Further work would be in order from these provocative cases. This might include research with a more careful statement of the dependent variables of political change and alternative hypotheses for political change other than those from the socioeconomic set alone. Rosenberg does zero in on several important trends, and a very fascinating idea, like many in the book, is left undeveloped. The trends include the breakdown of traditional social-economic-political institutions, the intensification of disparity between rich and poor, and increased economic concentration and centralization of political authority. The tantalizing idea is that many studies of modernization focus on the symptoms of modernization (urbanization and industrialization) rather than on the rural roots. He further notes the linkage between agricultural development and industrialization.

While the book reflects the strength of area-oriented scholars—their interdisciplinary focus and depth—it also reflects their weakness—theoretical development. This book does not come to issue with the Rudolphs' proposition that tradition adapts to modernizing functions. Further, it fails to grasp some of social stratification theory's implications about peasant frustration and political change. Nor does it acknowledge the important theory about the linkage between the failing neocolonial economy of pre-martial law days and the decline of traditional institutions. This is clearly an important corollary of the linkage between economic concentration and the decline of the traditional political machine. While one cannot help but feel stimulated after reading these studies of pre-martial law Philippines, one gets the same feeling that follows a reading of the Rudolphs' excellent study of *The Modernity of Tradition in India*—that the cases were pre-judged. If there is any point that both cases bring home, it is that change must run its course before it can be properly understood. This is particularly so where judgments are being made about the linkages between traditional and modern institutions; and between social stratification and political outcomes.

DAVID F. ROTH

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Political Elites in the Middle East. Edited by George Lenczowski. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1975. Pp. 227. $9.50, cloth; $3.50, paper.)

This paperback offers more quality interpretations of the political systems of leading Middle Eastern countries (Egypt, Iran, Iraq, Israel, Lebanon, Syria, and Turkey) than any comparably-priced book on the subject. While it deserves wide use in courses on Middle Eastern politics, it fails, inevitably, to further the systematic and comparative study of political elites because Lenczowski "felt that it would have been erroneous to impose a rigid methodological pattern upon the authors" (p. 8). We are consequently left with six competent and informative studies (excluding Gordon Torrey's brief and marginal piece on Syria) focused on political elites defined in different ways.

From a theoretical standpoint, Robert Springborg's interpretation of intra-elite relationships in Egypt is the most original and stimulating contribution in the volume. Others may criticize him for not defining the political elite rigorously enough, but such an academic exercise would be fruitless in discussing Egypt. Such a definition would require either strong political institutions or some combination of reputational and decision-making data which no one could obtain. Instead of trying to predict political attitudes and behavior from socio-economic or educational characteristics, Springborg plunges into an analysis of intra-elite processes. In the absence of strong organized groups or institutionalized political procedures, elites in Egypt are bound vertically by patron-client relationships of various sorts. The closer to the presidential patron one comes, the more fragile one's vertical connections are likely to be. Horizontal ties between elite members are also fragile, consisting of circles of friends, so-called *shillas,* which are in effect alliances of patron-client clusters. These alliances, especially at the top, are usually overlapping and subject to division by patronage from above. Springborg argues that "the present fluditiy of shillas prevents conflict from becoming crystalized and keeps personal hostilities from creating unbridgeable intra-elite cleavages" (p. 104). Their fragility, coupled with that of the patron-client networks at top levels, also explains the elite's relative importance in coping with Egypt's pressing economic problems. To stay in power, the chief patron balances his clients so as to prevent the shillas from hardening into organized factions, but does so at the cost of any strong vertical linkages that might mobilize the bureaucracy into taking and implementing complex policies.

Analytically, Springborg seems on firmer ground than James Bill, who relies on the *dawrah,* the Iranian version of a shilla, to cover both horizontal and vertical linkages among elite members and between the elite and non-elite. "The dawra network," he argues, "spans the entire class structure, working to close gaps, soften confrontation, and siphon off discontent at all levels" (p. 19). Bill then goes on to argue that the Iranian political elite is "a remarkably cohesive group" because "the patrimonial leader . . . has served to anchor the elite. . ." (p. 28). Vertical patron-client relationships are thus smuggled back into the picture. The Shah engages in the same balancing act Springborg ascribes to Nasser and Sadat, but Bill does not acknowledge its costs for Iranian policy-making. Do the vast petroleum revenues cement the Shah's networks into tight tentacles for controlling and implementing public policy or do the shiny new development projects simply divert attention, as they did for a while in Nasser's Egypt, from an underlying political and administrative stagnation? This is a provocative question, which only time will answer, but Bill's essay is disappointing on other grounds. Though he obviously knows a lot about the Iranian elite, he has tried to demarcate it artificially by "a combined positional, reputational, and decisional approach" (p. 22) which is never spelled out. Since, as he admits, he is "more interested in patterns than . . . in faces" (p. 22, n. 12), he would have been better off with Springborg's less pretentious approach. After locating his 375 positions and 250 incumbents, Bill then discussed the educational and family background characteristics of other, institutionally defined samples.

Instead of focusing upon an elite at any one time, Frederick Frey relates the background and attitudes of the Turkish elite to political development over the past half century. But since 1950, "elite-mass relations became critical and elite-elite relations are compounded and often confounded by this fact" (p. 73). Consequently Frey concludes with an analysis of elite-mass linkages, including recent Turkish elections. Had he hypothesized relationships between the changing bases of support of the two major parties and some of the dimensions of elite political culture discussed earlier, his complex argument might have been tighter.

I cannot here do justice to the careful and informative analyses of the Iraqi, Israeli, and Lebanese elites by Phoebe Marr, Emmanuel Guttmann and Jacob Landau, or Iliya Harik. Suffice it to say that each article, like the three discussed above, stands in its own right and contains original research, but that the potential here for comparative analysis has not been developed.

CLEMENT HENRY MOORE

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Political Culture and Group Conflict in Communist China. By Alan P. L. Liu. (Santa Barbara, Calif.: American Bibliographical Center, Clio Press, 1976. Pp. xvii + 205. $17.95, cloth. $5.75, paper.)

It is exactly ten years since the Great Proletarian Cultural Revolution (GPCR) broke loose in China in 1966. Initiated by Chairman Mao Tse-tung of the Chinese Communist Party, the GPCR was aimed, among other things, at shaping Chinese society according to Maoist revolutionary values and removing from power such "capitalist powerholders" in the party as Liu Shao-ch'i (then China's head of state) and Teng Hsiao-p'ing (the general-secretary of the party), who allegedly were pursuing a counterrevolutionary revisionist line to restore capitalism in China. But once started, the GPCR developed a momentum of its own, and no one including Chairman Mao himself proved capable of controlling its pace and direction. During 1966 to 1968, millions of rebellious youths organized themselves into hundreds of Red Guard and "rebel" groups, rampaged about the country to "destroy the four olds" (old ideas, old culture, old customs, and old habits), and staged massive demonstrations in Peking and in the provinces against the "capitalist powerholders" to seize political power as well as to obtain other economic and social rewards. At the height of the GPCR, public order in China was in shambles as large-scale, bloody fighting erupted between rival groups and, in many instances, between Red Guards and units of the People's Liberation Army.

The book under review, *Political Culture and Group Conflict in Communist China* by Alan Liu, a member of the political science faculty of the University of California (Santa Barbara), is about the various socioeconomic-political forces that brought forth the intense and violent group conflict in the GPCR. The book is not a chronicle of the GPCR; rather, it is an anatomy of the group conflict and is highly conceptual in tone. Part I of the book provides an analytical framework for the book and discusses four components of the group conflict: sources of conflict, conditions for conflict mobilization, factors determining the conflict process, and the conflict resolution. Part II identifies the participants of the conflict (i.e., students, workers, peasants, and veterans) and the issues of contention, and examines the demand, motivation, and behavior of each conflicting group. Part III analyzes the resolution of conflict, describing how the Chinese leaders managed the group conflict and reimposed control over Chinese society.

The source materials of Liu's study—Chinese radio broadcasts, Red Guard publications, and witness accounts—are quite familiar to China specialists; however, the book is somewhat different in linking the study of Chinese politics with general social science concepts; more specifically, he has examined group conflict in China by applying theories of social conflict developed in Lewis Coser's *The Functions of*

Social Conflict, Ralf Dahrendorf's *Class and Class Conflict in Industrial Society,* Anthony Oberschall's *Social Conflict and Social Movements,* and by other social scientists. In so doing, he has presented additional insights into Chinese political behavior and provocative interpretation of group conflict in the GPCR.

For example, Liu sees the political culture of the post-1949 China as one of the two sources of group conflict in Chinese polity. According to Liu, the accentuation of politics for the young in China, e.g., the heightening of political sensitivity, class consciousness, and a "statist" tendency, etc., disposed them toward political involvement in the GPCR, and tended to emphasize conflict as the norm of interpersonal relationships and to legitimize aggressive acts and violent conflict. The systematic inculcation of dual standards of political civility—aggression against "the enemy" and comradeship with "the people"—through regular school curricula, introduction of a new political vocabulary, and specific mass campaigns is said to have also accounted for a tendency toward youth factionalism and group violence.

Liu also perceptively points out that such factors as division among the elites, loosening of social control, precipitating events, the availability of focal points and communications are necessary conditions for mobilization for group conflict. The presence of these conditions, reinforced by large numbers of conflicting groups engaged in what is called "induced conflict" (which is less amenable to compromise) and using such means of conflict as rifles, machine-guns, tanks, and other deadly modern weapons, according to Liu, not only greatly prolonged and intensified the group conflict during the GPCR but also rendered it extremely violent.

There is one crucial question which Liu has not dealt with. Namely, the GPCR is in many ways an anomaly and provides only a very limited time span from which to examine group conflict in China; thus, can the generalizations on Chinese politics derived from the GPCR be applicable to other periods? One could also question to what extent the conclusions which are true for Communist China are also true for other Communist systems. For example, the author states that Communist systems centralize "institutionalization of group conflict," but inwardly "are characterized by uninstitutionalized conflict," and "consequently there is an increased probability that sudden and violent conflicts will take place in Communist nations, particularly in those undergoing rapid social and economic changes" (p. 5); yet, so far only one Communist state—China—has experienced the sudden and violent conflict.

There are several other minor criticisms. Perhaps through oversight, the author has not specifically defined what he means by "group conflict" in the book; given the weight of that concept in the whole study, a clear definition is undoubtedly in order. Statements on the violent conflicts in the GPCR notwithstanding, the author has provided the readers with only a few "body counts" (for instance, pp. 81, 85, 87, 91); I wish he had used more casualty figures, instead of mere words, to convey to the readers the severe and widespread violence that took place in China during the GPCR. In addition, the names of several Chinese leaders, K'ang Sheng, Chang Ch'un-ch'iao, and Ch'en Po-ta, are misspelled. It is puzzling that most Chinese officials, including Lin Piao, are not indexed.

In spite of the criticisms raised above, I still rate *Political Culture and Group Conflict in Communist China* a stimulating work, valuable not only to students of Chinese politics but also to those interested in comparative politics.

PARRIS H. CHANG

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The Government and Politics of Hong Kong. By Norman J. Miners. (Hong Kong: Oxford University Press, 1975. Pp. vii + 300. $13.75.)

Once a base for Western military expansion in East Asia, Hong Kong now performs the role originally envisioned for it in the Nanking Treaty of 1842: a treaty port which facilitates Chinese-western commerce. While its government is no longer classic "colonialism," nonetheless four million Chinese remain governed by a British governor. Its continued existence depends upon Peking's suffrance and London's willingness to administer the territory, as well as the populace's tolerance of its inequities.

The system persists because it pays off. In recent years, China has earned between 15 and 30 percent of its foreign exchange there. A high percentage (27 percent in 1973 but now reduced) of the Bank of England's gold and foreign exchange reserves came from Hong Kong deposits. Within Hong Kong, the population—many refugees from the People's Republic—saw the economy expand 8 percent annually from 1966 to 1973, while real wages increased 4 percent per annum. And since the mid-1950s, over 1.8 million people have been sheltered in the huge, noisy, crowded government housing estates.

Norman Miners, senior lecturer at the University of Hong Kong, describes well the political system which has emerged in and sustained such a setting. Miners does not analyze how a British system has been adapted to and shaped by the Chinese cultural milieu. Although brief chapters on pressure groups and public opinion portray the interaction between government and the populace, the book's strength is elsewhere.

The book's excellence stems from its balanced, masterful account of complex, formal institutions. This is traditional political science at its best. In lucid and concise style, Miners details the role of the governor, the financial system, the civil service, and local government. Perhaps a lengthier treatment of the secretariat and its key departments—the heart of the system, in my opinion—would have been desirable, but these were in process of reorganization as the book was being written. In subtle British style, Miners also makes many informed judgments about Hong Kong's future, the possibilities of governmental reform—a recurring topic in the city—and the impact of such decisive people as John Cowperthwaite and Murray MacLehose.

In sum, this book joins the growing list of indispensable works on Hong Kong by Endacott, Hopkins, Jarvie and Agassi, England and Rear. It illuminates well one of the few political remnants of a governmental form that but a generation ago dominated the world and was central to comparative government.

MICHEL OKSENBERG

University of Michigan

The Rise of Afrikanerdom: Power, Apartheid, and the Afrikaner Civil Religion. By T. Dunbar Moodie. Perspectives on Southern Africa No. 11. (Berkeley: University of California Press, 1975. Pp. xii + 328. $12.50.)

South Africa has been termed "the beloved country" by the world-famous author, Alan Paton, but it is also one which has been analyzed, interpreted, reinterpreted, and regarded with something less than love by the overwhelming number of United Nations General Assembly delegations and African diplomats. With the independence of Mozambique and the dispatch of South African armed forces to Angola, if only for a brief period, South Africa is very much in the public eye. The South African white community is mesmerized by studies about South Africa, whether by Britons, Africans, Americans, Canadians, or even South Africans themselves; whether such studies become banned books in South Africa is an even more exciting question. However, the surfeit of studies on South Africa is quite misleading from two points of view. First, there are too few American publishing houses which publish anything about this far-off nation and prefer to stick to texts that sell well in established undergraduate and/or junior college markets (see the provocative article, "College Textbook Publishing in the 1970's" by Phillip Whitten, a student of South African affairs, in the *Annals of the American Academy of Political and Social Sciences*, 421 [September, 1975]). The American area specialist thus often has to turn to British publishing houses for the newest works on South Africa. Second, amidst the plethora of books on South Africa there is an appalling amount of dross. Much of the underlying reason for this is that such books were written to attack or defend the political system of apartheid, to persuade and proselytize, and to legitimize the prevailing order. The issue of race is so heavily laden with emotional freight that it is exceedingly difficult not to be engulfed by the power of passion. Nonetheless, there is a place for such work; we need to avoid the sterility and deceptiveness of what could be termed "value-free" research.

Thus it is a pleasant and reassuring sight to see the commitment of the University of California Press to this Southern African series. This places quality work before American audiences, often at reasonable prices because several in this series have been published in paperback. *The Rise of Afrikanerdom* is written by a South African scholar originally educated at Rhodes University in Grahamstown who subsequently attended Oxford and Harvard Universities. Moodie is now a Professor of Sociology at the University of the Witwatersrand, the English-medium university in Johannesburg. It appears from the text (pp. xi–xii) that the book was originally a doctoral dissertation, but Moodie does not mention the date of the dissertation or the university. If one may assume that it was written at Harvard, it seems quite puzzling that the author would have overlooked John A. Templin's 1966 doctoral dissertation there, namely, "Permutations of the Idea of the Elect People in South Africa: A Study in the Evolution of a National Consciousness." Moodie's book is slightly marred by occasionally poor typesetting which should be corrected in the next edition. The index also needs greater rigor and breadth so that this fine volume could double as a reference work for the research student. The bibliography, however, is quite thorough, with full citations, and contains a most helpful prefatory section which surveys some of the more arcane sources.

The book itself is essentially a brilliant exegesis of how the Afrikaner perceives himself and the duties and privileges that accompany such perception, and Moodie draws upon theology and sociology to enrich this exegesis. The mode of analysis is diachronic and is restricted almost entirely to the Afrikaner elite, rather than the Afrikaner mass. Moodie explores the mystique, the organizational weaponry, the rituals, the sacred history, and the doctrinal disputes of Afrikanerdom in a remarkably thorough and even-handed manner. His data is unusually well organized, his line of reasoning lucid and compelling, and he writes with commendable grace. The book reveals an impressive grasp of knowledge of South African politics and enviable skill in handling the nuances of the politics of Afrikanerdom. He has included a theoretical and methodological appendix, which he might better have transposed to the beginning chapter. The book's philosophical and humanistic tendency make it a most welcome companion to the study by Hendrik W. van der Merwe et al., *White South African Elites* . . . (Cape Town: Juta & Company Ltd., 1975) which deals with positional elites (including a considerable number of Afrikaners) and utilizes survey data and the concomitant statistical operations. *The Rise of Afrikanerdom* does make use of interviews (pp. 180, 188 n. 11, pp. 183, 250, and 257 n. 22) yet the author fails to mention how many Afrikaners he interviewed in the course of his study and the nature of his interviews.

Moodie could have been more specific regarding interview techniques and sample sizes, but this minor flaw is more than offset by the high quality of the archival material that the author was able to locate in Cape Town and Potchefstroom and by his diligent examination of the twentieth-century Afrikaans press. Such scholarship will undoubtedly appeal to those either well-versed in South African politics or primarily interested in the comparative study of nationalism and ethnicity. This study, one of the best and most provocative works on Afrikanerdom, demonstrates the author's remarkable skill in compressing vast amounts of both familiar and new data into the organizational framework of what he terms civil religion and ideology (as defined on pp. 296 and 298, respectively). This intellectual operation enhances the explanatory and predictive power of the author's study, something which sets it aside from much of the literature on South Africa.

RICHARD DALE

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Legal Reform in Occupied Japan: A Participant Looks Back. By Alfred C. Oppler. (Princeton, N.J.: Princeton University Press, 1976. Pp. xx + 345. $20.00.)

This is a useful book; it sketches the background of Japan's unique postwar reforms and captures the tone, atmosphere, and personalities in a most enjoyable way. Much of Oppler's information is otherwise unavailable in a reliable organized form, but most valuable are his informed opinions on issues of the day, found on practically every page with refreshing clarity and integrity. You know exactly where he stands whether you agree or not. The treatment is more episodic than analytical; indeed much of its charm comes from the reminiscences of this former German jurist of broad experience and realistic liberal faith inspired by escape from Nazi Germany (1939) and naturalization (1945) in the United States, which he really knows to be (and therefore many times calls) the "land of the free."

In the introduction Oppler explains his departure from Nazi Germany after losing his position as an associate justice of the Prussian Supreme Administrative Court *(Oberverwaltungsgericht)* for "racial reasons, even though he was baptized and brought up a Protestant and was married to an "Aryan," should some bigot feel such facts relevant. After coming to the U.S., he learned English and soon after the war ended went to Tokyo as Chief of the Court and Law Division, Government Section, Supreme Commander for the Allied Power (SCAP). Arriving for work February 23, 1946, just after the SCAP draft of Japan's new constitution had been completed in Government Section, though it was not yet passed by the Diet or effective (1947), he served in various positions, mostly implementing legal reforms in Japan, until his retirement in 1959. This much biography is important because it shows, despite his lack of training in things Japanese, the unusual capacity of this "participant" to mediate between the wartime Japanese legal system of German and French derivation and American-styled reforms intended to democratize and establish civil liberties in postwar Japan.

The first four chapters describe Oppler's trip to Tokyo, the personalities and organizations within the Government Section. Chapters 5 to

10, the core of the book's legal discussion, describe the content of the legal reforms: the new constitution (1947), the reforms to implement it in the courts, the procuracy, the bar, the civil and criminal codes, and the procedural civil and criminal codes.

The next 15 chapters recreate chronologically the important events and projects related to these reforms, down to Oppler's eventual return to the U.S. in 1959. These include his experience in dealing with the legal affairs of Japan's only postwar cabinet led by the Socialist Party (1949); establishment of the Japanese Civil Liberties Union; women's emancipation efforts; the labor union movement; the peace clause; appointing the first 15 supreme court justices; regional inspection trips; the supreme court mission to the United States (1950); problems of the Korean conflict; removal of MacArthur and the end of the occupation. The book ends with a concluding evaluation of these reform efforts seasoned by intervening decades.

A sampling of the problems raised in the occupation discussed, and some of them resolved will surely whet the appetite of all interested in Japan in general, or legal reforms in particular: Was the new constitution necessary or was the Meiji Constitution (1889) adaptable to the democratic way? How could a new constitution be instituted? Could the Meiji Constitution be amended by the Diet? If so, could the Diet change the proposal (submitted by the Emperor) or could it only adopt or reject *in toto?* What was the actual role of SCAP—coercive, persuasive, or permissive—in the adoption of specific provisions of the constitution after the drafting was completed? Who was the originator of the constitution's famous peace clause (art. 9), Shidehara or MacArthur? What was the role of the Emperor? Are crimes of *lèse majesté* tolerable in a democracy? What should be the relationship between the Diet and the courts (art. 41 vs. art. 81)? Should there be Diet supremacy or dual supremacy? How could the Diet be protected from the overbearing legislative bureau of the past? How could judicial independence from the Ministry of Justice be ensured? What should be done about the separate administrative court (p. 100), well known to Oppler as justice of such a court in Prussia? How could the courts be protected from excessive surveillance by the legislature? What is proper (or improper) cause for legislative impeachment of judges (p. 96)? What could be done to insure a judge's tenure? At what age should justices retire (65 for lower courts, 70 for Supreme Court?) When is the prime minister's veto of a court injunction consistent with constitutional independence of the courts? Should judges be elected? May judges engage in political activities? If so, how much? May the Supreme Court change procedural statutes under the new rule-making power (p. 101)? How independent should the bar association be in determining qualifications, training, and discipline of lawyers (p. 108)?

With the constitution promulgated, the Japanese government and SCAP were faced with the task of changing continental-type codes—by then acclimated by a half-century of Japanese experience—to conform to extensive infusions of Anglo-American theory, procedures, and concepts, mainly in the area of civil liberties (more extensive treatment of this is found in several articles by him and his colleagues in vols. 24 and 25 of the 1950 *Washington Law Review*). Oppler briefly summarizes his problems with the codes: for example, what should be done about the authoritarian aspects of the Japanese family system? How should sex equality be implemented? Or, in civil procedures, should aspects of the American adversary system of trial be adopted?

In criminal procedures, how should police and procurator functions be separated (p. 138)? Is hearsay admissable? May the state appeal (p. 144)? May witnesses testify against their employers or relatives (p. 134)? Is truth a defense to defamation (p. 128)? In the criminal code few substantive provisions were changed except those on *lèse majesté*, but the Supreme Court has since struggled with an issue raised in SCAP regarding differential penalties for crimes against ancestors in the two patricide cases (p. 121).

There are a couple of important areas this book does not cover. Thus, the title of the book is overly broad in that Oppler is mainly concerned with "democratization" of the law, and therefore almost exclusively with the constitution, courts, codes, and procedures related to protection of civil liberties. Though admittedly these are the core of the program, still many important Occupation legal reforms of American design are barely mentioned. First, the several regulatory laws of particular importance are not adequately treated: the Securities Exchange Act; the Labor Relations Act, and the Marine Transportation Act; and the Antimonopoly Law. These acts all follow the American pattern in the name of democratization, and the program as a whole, plus the amendments to the Commercial Code, were of great significance in the economic life of Japan. Also the land reform and reform of the bureaucracy were of great political and governmental significance, but not really covered.

Yet much can be learned henceforth from these statutes about the efficacy of alien law to change a fixed social pattern or intrenched interest group. Here, the effect desired–indeed sometimes even the perceived problems–often turned out to be illusory; the ball took some odd bounces and only in recent years have some of these reforms experienced a revival because Japanese lawyers have found new uses for them, and Japanese students are coming abroad again to take new readings of the "mother law."

Second, the attempt to pursue this great legal reform from the *Roppo* (six codes) to the society is only touched on briefly at the end in a concluding evaluation, which is spotty, impressionistic, and derivative.

The foregoing two points do not come within the scope of Oppler's book, but they are of high interest to all students of Japan and comparative law in general. The legal reforms which Oppler treats were a unique experiment in induced democracy by importation of foreign laws. The effect of the experiment on Japanese society ought soon to be measured; but the enormity of such a scholarly task invites binational teamwork.

DAN F. HENDERSON

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Value for Money: The Hong Kong Budgetary Process. By Alvin Rabushka. (Stanford, Calif.: Hoover Institution Press, 1976. Pp. iv + 176. $9.95.)

This reviewer shares Alvin Rabushka's fondness for Hong Kong and enjoyed the nostalgia of reading his book. Understanding it, however, depends so much on his earlier one (*The Changing Face of Hong Kong*, 1973), that a revised and expanded version of the first volume would have been a wiser course of action than the publication of a new work.

Rabushka spends a good deal of time discussing the colony's constitutional and administrative framework. Control rests with civil servants who mainly come from the United Kingdom. Citizen participation in the decision-making process is a farce. The Urban Council has jurisdiction for minor tasks and is elected by a handful of eligible voters. Absolute power rests with the governor of the colony, with the Executive Council as his advisory board. The governor makes laws with the advice and consent of the Legislative Council, whose official members outnumber the appointed "unofficials" so the administration's measures are guaranteed passage. The "unofficials" may talk up but they largely play a passive and formal role in the legislative process. Again the unofficials play a compliant role on the finance committee, the standing committee of the legislative council which considers budgetary proposals before they proceed to the Council. The public works and establishment subcommittees consider public works expenditures and salary requests.

The bureaucratic structure of Hong Kong government is coordinated by the Colonial Secretariat. Before recent revisions, the "general" division of the Secretariat consisted of policy branches, each of which coordinated a number of departments. The Financial Secretary heads the financial and economic division; nominally, he ranks behind the Colonial Secretary but Rabushka emphasizes his dominance of the budgetary process.

Nor is the recent reorganization emerging from a McKinsey management report likely to affect the balance of power. Six new Secretariat branches were created in 1973, replacing the old policy branches. The purpose of this change was ostensibly to achieve policy coordination across departments. The underlying motive, however, was to increase the departments' power vis-à-vis finance. Rabushka sees the constitutional and administrative framework as assuring the failure of this plan.

The dominance of the Financial Secretary is not merely a question of the controller of the purse strings calling the policy tune. Rather it seems to have been the achievement of outstanding personalities more brilliant than their peers. The book contains extensive quotes from the speeches of the last three Financial Secretaries–Clark, Cowperthwaite and Haddon-Cave. Their key importance is corroborated in a number of interviews with leading administrators. Rabushka proclaims that his 50 interviews demonstrate the scrutability of the Hong Kong government, contrary to the general impression. However, judging by the published excerpts, the interviewing yielded less than sensational results. Rabushka would have made a greater contribution had he penetrated the government's statistical curtain. Hong Kong is notorious for its lack of hard statistical data, e.g., balance of payments data.

Rabushka commends the Financial Secretaries for their noninterventionist policies, ignoring the changes discussed in his earlier book. For one well-known writer on Hong Kong (Richard Hughes), the password is now "discreet guidance" instead of laissez-faire. Budget balancing (rather budget surpluses) is praised, but again the picture is changing, with deficits in the 1974–75 and 1975–76 fiscal years. In years of surplus the tight rein on "recurrent expenditures" contributed to recruiting problems for the civil service, possibly explaining the lack of worthy adversaries for the Financial Secretary.

The Financial Secretaries (most notably Cowperthwaite) have seen economic growth as depending on noninterventionist policies and budget balancing. But this is not necessarily so. A similar city-state, Singapore, has been challenging Hong Kong's financial preeminence via its financial reforms, including a central bank. Discretionary monetary and fiscal policy might help to combat Hong Kong unemployment. The spectre is of course inflation, but the latter should not be given all the weight in an economy so full of human misery.

Rabushka's book would have benefited from a wider focus. One suspects that there is more interaction in economic policy making with mainland China, the UK government and local business interests than is here revealed. Again, some discussion of this is found in his earlier work.

As a supplementary volume, I would like to recommend as good value the *Hong Kong Annual Report 1976* (Government Printer, Hong Kong, 1976, HK $15.00 which equals US $3.00). This volume contains an extensive discussion of taxes surprisingly neglected in Rabushka, plus 22 chapters on all aspects of Hong Kong life, including constitution and administration, and the pictures are dazzling.

JACOB COHEN

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Politics, Bureaucracy and Rural Development in Senegal. By Edward J. Schumacher. (Berkeley: University of California Press, 1975. Pp. 303. $18.75.)

Politics, Bureaucracy and Rural Development in Senegal is a solid comprehensive study on the evolution of party and state bureaucracy in Senegal since gaining independence in 1960. It stresses rural development administration in particular but governmental and party changes at the national level as well as the transformations of agencies responsible for rural programs at the national, regional and local levels are discussed at length and in detail.

The purpose of Schumacher's work is to assess the continuity and change taking place in the above-mentioned institutions over the last 16 years. Ultimately the author concludes that continuity rather than change characterizes the party.

Regarding the policy of the government, Schumacher traces a gradual shift away from an emphasis on socialism and towards the pursuit of pragmatic economic development goals. He comments:

> . . . The demise of the Animation and Expansion Department, and the local development structures it promoted, entailed abandoning the operational substance of the government's commitment to rural socialism as initially conceived. As Senegal turned toward the 1970s, official rural policy was decisively turning away from its original commitment to promote the diffuse goals of egalitarian, community-oriented social change and the kind of increased popular participation inherent in the concept of animation rurale. In its place, rural development policy and organization were moving toward a new reliance on maximum administrative efficiency and economic incentives aimed at individual producers which, along with the improved delivery of technical extension services, would increase the regime's capacity to achieve measurable production goals (p. 218).

Senegal is a country which has excited considerable interest among American researchers and consequently a large body of literature has appeared. Schumacher is well-acquainted with studies of Senegal in English and in French and has managed to summarize and analyze results of earlier works without repeating excessively what others have done. Indeed, the advantage of this book is the perspective it provides on 16 years of the development process. Its disadvantage, of course, is that it is difficult if not impossible to transfer the factors operating in this case to any other country. Thus only those interested in Senegal find real insights in this volume although there are questions raised here which may be usefully applied elsewhere. A further detraction from the book is the confusion of using a number of acronyms reflecting the proliferation of agencies charged with rural development. If the reader is not careful, one is unable to follow exactly who is replacing what with whom in the various rural reforms discussed throughout the last chapters. This drawback, however, is in the nature of the subject so the criticism is perhaps unfair. This book is a generally well-written and clear exposition of a complicated subject bringing up to date many of the early efforts to understand Senegalese development.

LUCY CREEVEY BEHRMAN

University of Pennsylvania

Japan in Crisis: Essays on Taishō Democracy. Edited by Bernard S. Silberman and H. D. Harootunian. (Princeton: Princeton University Press, 1974. Pp. 469. $17.50.)

The Taishō era, 1912–1926, in Japan's history, though brief, has a flavor all its own; for those were the years when the Japanese industrial revolution reached maturity and when society began to be predominantly urban rather than agrarian. Moreover, during this period there occurred that sprouting of seemingly genuine democratic activity, politically and socially, which was to wither away in the course of the 1930s. It was a restless era; when weakness at the top—the Emperor Meiji's successor being mentally unstable and the Satsuma-Choshu clique rapidly losing its old hegemony—was matched by a variety of challenges from below; from the *narikin* (nouveaux riches), from the intelligentsia of the Left, and from a new generation of Diet politicians. Shortly before the end of those 14 years there took place that appalling disaster, unparallelled in recorded history, the great Kanto Earthquake, which destroyed what remained of metropolitan Yedo. It is no wonder that many elderly, conservative, Japanese were half inclined to accept the vulgar superstition that the disaster had been caused by the giant catfish, who lived under the surface of Japan, heaving his back in fury at the selfish folly and wickedness of the human beings above him. For the ideal of *wa,* "harmony," was never less in evidence than during the socially disturbed years of the Taishō era.

No better study of those years, in the English language at any rate, has been published than *Japan in Crisis.* Professors Silberman and Harootunian are to be congratulated on bringing together a clutch of scholarly articles, including their own contributions, that admirably illustrate and explain different currents of political and cultural thought characteristic of Taishō Japan. There is rich fare here for the specialist, and for the historian and political scientist primarily concerned with other parts of the world.

It is interesting, for example, to find Professor Iriye in his article on "The Failure of Economic Expansion" underlining the significance of the defeat of the racial equality clause at Paris in 1919. For it has often been argued that Japan's proposals on racial equality amounted to no more than a bargaining point to be traded against the issue of Shantung. But Iriye maintains that the defeat of the racial equality clause shook Japanese opinion very profoundly.

> Without the West's acceptance of the principle of equality of treatment, the emerging world order seemed to hold out little hope for Japan. The West, it seemed, had denied Japan the right to peaceful and economic territorial expansion. How Japan should behave in such a seemingly hostile world, in which it appeared to be completely isolated, became a major concern of opinion-makers as well as of officials (p. 258).

Again, scholars of rural history may be inclined to pay greater attention to Japan after reading Ann Waswo's contribution on "The Origins of Tenant Unrest." There they will discover a revealing example of that topsy-turveyism which all Western students of Japanese society come across sooner or later. It is notorious—Ireland in the eighteenth century being a striking illustration of this—that tenants prefer resident to absentee landlords. For an absentee landlord will often allow a bailiff or overseer a freedom that can lead to minor or major exploitation of tenant cultivators. Just the opposite was the case in Japan. Ann Waswo points out that in Japan absentee landlords were generally more willing than resident owners to meet tenant demands for rent reduction.

> Resident landlords bought and sold land frequently. Typically, the new owners raised rents when they took possession and sometimes evicted current tenants in favor of their own. In contrast, absentee owners rarely acquired new holdings; their tenants enjoyed greater security, farming the same plots for many years in succession (p. 391).

Finally, the serious reader will be enlightened by the important study of "Taishō Democracy as the Pre-Stage for Japanese Militarism" (Kato Schuichi). For Professor Kato shows that what followed the Taisho era was not a shift from democracy to fascism, but rather a movement "from a phase of liberalization to another phase of bureaucratization and militarization within a political structure which had not basically changed" (p. 236). In other words, the Taishō years marked a turning point on which Japan failed to turn. Thus, there is a certain pathos about that era; and this is implicit in most of the articles that comprise *Japan in Crisis,* a book that is something of a landmark in the progress of Japanese studies in the western world.

RICHARD STORRY

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Political Education and Stability: Elite Responses to Political Conflict. By Ted Tapper. (New York and London: John Wiley, 1976. Pp. x + 265. $17.95.)

This slender volume consists of a series of "case histories" or analyses of current movements in Great Britain and the United States viewed from a political socialization perspective. It is essentially a study of how governments create conflict by creating inequalities and then try to contain this conflict by "socializing" to accept and justify inequality. "One of the central themes of this book has been to show how socialization processes try to fulfill two somewhat contradictory functions: to build political consensus and to ensure role differentiation within the consensus. It is especially hard in a democracy, with its universalistic value system, to justify an inegalitarian division of political labor, and yet this is an intrinsic feature of both the British and American models of representative democracy" (p. 241). Since the author rejects the notion of the "oversocialized" citizen, he holds that such socialization to inequality is fraught with tension and serious conflict. It is his contention that the consensus and stability said to exist in these two democracies is therefore more fragile and illusory than usually assumed.

To make this point he studies certain unreconciled or at least dissatisfied subgroups (such as Afro-Americans in the United States, Roman Catholics in Ulster, students and women in both countries). He also devotes several chapters to political socialization (or political education as he chooses to call it), especially as it takes place in the schools. Much of what he has to say about these topics is insightful, some of it provocative, although some materials traverse well-trodden grounds. Thus, for example, he joins the chorus of those who accuse the American civics curriculum of being bland, conservative, and nonpolitical; a criticism the schools richly deserve. He does not, however, mention the recent attempts at curriculum revision designed to meet this criticism. It is also surprising—in view of his emphasis on the importance of structure—that he does not examine the structure of the school and the extent to which this violates the school's avowed citizenship goal.

The chapter on women is perhaps the weakest in the book, no doubt because, as the author himself concedes, "in a few hundred words it is impossible to discuss adequately the women's liberation movement in Britain and the United States" (p. 203).

The chapter on divided Ulster, on the other hand, is a superb case study. Tapper shows how the segregated communal arrangements of the Protestant and the Catholic communities enhance hostility and suspicion between the two and make sensitivity to each other virtually impossible. He documents that the instructional process in the two school systems feeds information designed to reinforce this state of affairs. He offers valuable observations on the actual process of socialization. This type of a case study applied to other situations and other populations and subpopulations would be a direction into which political socialization research could go with great profit.

A chapter which could have benefited from a similar socialization approach is that on political elites. Sufficient literature exists on that topic to illustrate the variety of socialization processes which propel people to seek political leadership. When discussing how the legislators become socialized to the institution, Tapper at times seems to confuse political socialization with the learning of the rules as distinguished from internalizing them. The fact that "mavericks will want certain things from the institution and the price they have to pay is working within the rules" (p. 90) can hardly be considered an example of political socialization. It is also regrettable that the author did not speculate on some related topics such as; can political socialization account for the fact that legislators perceive their roles differentially? An equally important question might have been: Who do the ablest people in the United States tend to go into the corporate world or the world of enterprise whereas in the United Kingdom there is a tradition of public service?

The chapter on working-class conservatives, while going over much familiar ground, is an important contribution in that Tapper tries to refute many simple explanations of the deferential worker. Instead he looks at structural and personal variables which might account for the presence of working-class people in the Conservative party. The emphasis on structure, especially in the work place, is one which adds a good deal of insight into a phenomenon which has usually been discussed rather superficially. His concept of the "privatized worker"—in distinction to the deferential one—merits closer attention and possibly might also offer a good explanation for blue-collar conservatism in the United States.

Among the strengths of this book must be counted Tapper's attention to the impact of social and political structure on socialization experiences. Another important contribution is the focus on adult socialization. This focus has been badly neglected in favor of the nursery and the school; yet it is conceivable that adult experiences contribute equally, if not more, to industrial man's political socialization. It is more difficult to concur with the author's main conclusion that the presence of conflict and tension in both countries (resulting from structured inequalities) indicates that the two countries do not really enjoy genuine stability. Such a conclusion makes stability a static state without conflict and without the ability for change or adjustment. That to me is an untenable definition of the term. Conflict is the very stuff of politics. A system's stability should be judged by citizen willingness to solve conflicts within the confines of the system, in distinction to those systems where the only way for change to take place is by tearing down the entire system. Tapper, for example, cites the student movement and especially its fight for civil rights as an example of lack of consensus. One could retort that the civil rights movement represents the very opposite. In the first place, students did not intend to tear down the system and, in the second place, in opposing discrimination they were advocating a consensus which on a general verbal level was subscribed to by most Americans although it was certainly violated in practice. We could, therefore, argue that these students have been socialized very well indeed to the values of the American system (Etzioni refers to such protests as "demonstration democracy").

In summary, while the author's conclusion with respect to stability seems debatable, the general theme of the book is sound indeed, namely that political socialization cannot be separated from the structure in which it takes place. In making this point the author indirectly alerts us once more to the fact that we have as yet failed to develop a genuine theory of political socialization and have developed (?) inadequate observational processes for discovering it.

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Dissent in the USSR: Politics, Ideology and People. Edited by Rudolf L. Tőkés. (Baltimore: The Johns Hopkins University Press, 1975. Pp. 453. $15.00.)

This substantial volume opens by stating its goal, which, in the words of its editor Rudolf L. Tőkés, is "that of helping to initiate a collective, scholarly inquiry into the various manifestations, the socio-political significance and the prospect for dissent in the Soviet political system" (p. x) but closes on a seemingly contrary idea expressed in the twelfth and final essay by George Feifer, "that Soviet dissent cannot be seen in anything approaching true perspective without somehow accounting for the 99.9% who have so far abstained" (p. 240).

It may well be that the authors did not realize this apparent paradox in their volume's argument, but there are good, objective reasons for it in the very character of the phenomena they investigate. "Dissent" and "the democratic movement" still preoccupy scholars and other western observers, but the authors of this work all seem to agree that they were writing a kind of requiem for the whole dissent phenomenon in Russia. Most of them didn't feel sure about the chances of resurrection of widespread dissent and political changes, unless it were to occur "several generations ahead," as Theodore Friedgut put it (p. 134).

In fact, the very concept of "democratic movement" which many observers used in describing the dissenters of the 1960s and the early 1970s, is not agreed upon. Some of the authors in this volume talk about "political opposition," others argue that the whole phenomenon consisted of sporadic and unconnected expressions of protest by very small groups and by individuals—not a stable, structured organization which would justify the term of "movement."

The difficulty in assessing Soviet dissent is compounded by the unsatisfactory state of current theorizing and understanding of the deeper trends working behind the "happy" facade of official Russia. Tőkés is right in expressing his dissatisfaction with existing western models and concepts and claims that the documents now available from *samizdat,* as well as the contradictory and multifaceted phenomena and opinions they reveal, help us to reconsider outdated views. But he does not make it clear what exactly has to be reconsidered and what is made outdated by the *samizdat* production. After all, as the volume shows, destalinization seems to be stopped, the more outspoken manifestations of dissent (which were triggered precisely by the system's tendency to block destalinization) seem successfully muzzled—and all is almost back at square one. Did the West exaggerate the importance and depth of political protest of the late 1960s, as Feifer implies? Or were these manifestations but a harbinger of things to come?

The volume is a welcome contribution to our thinking about dissent and, to a less extent, about the Soviet system. Although the authors had difficulties in assessing Soviet dissent, they offer an exhaustive picture of ideas, people and events, the unfolding of protest, and its suppression by the regime and its police. The editor's introduction and three chapters by Frederick C. Barghoorn, Walter D. Connor and Gayle Durham Hollander try to locate these phenomena in a general framework concerning the Soviet system at large. Two authors, Theodore Friedgut and George Feifer, offer studies based on samples; Friedgut interviewing 40 recent emigres to Israel, and Feifer drawing on the observations of an unknown number of Moscow intellectuals. Feifer's subjects are quite well-informed but not involved in active protest; sometimes they even criticized the active dissenters (though not from official positions).

Other authors deal with different aspects of dissent. Thus, Howard L. Biddulph analyzes the methods used by protesters; Barbara Wolf Jancer studies the religious (partly also national) causes of protest; Gene Sosin presents the uncensored songs *(magnitizdat)*; Robert M. Slusser dwells on historical writings of internal critics; George L. Kline studies the critical philosophers; Peter Dornan offers a profile of Sakharov; and Peter B. Maggs helps with the technical intricacies stemming from the Soviet adherence to the copyright convention.

The overall result is, undoubtedly, a good compendium on dissenters and protesters. As to the significance of protest and prospects for political changes, the authors are hesitant to say. Barghoorn took a long, historical view; having surveyed the deeply seated impediments to liberalization in Russia, he found that the leadership is shrewd enough to deal with the protesters efficiently. However, the causes of dissatisfaction remain and are not dealt with as efficiently; therefore frustrations and protest will keep building up and will need to be suppressed.

Connor believes that the difficulties encountered by political institutions in mastering the growing social complexity account for manifestations of protest but he feels that the existing control-prone and centralized system manages to care for diverse social interests, whereas the dissenters, though a product of 50 years, "failed to strike a responsive chord among the masses" (p. 155). The deeper reason for this may be that the Russian masses show little interest in freedoms and rule of law. Only in long-term broad changes in structures and attitudes is there hope.

Hollander, on the other hand, gives little importance to the question of support for the dissenters among the masses because workers never played an important decision-making role in tsarist and Soviet Russia. For her the key lies in the flow of communication, the rising expectations among the intelligentsia to have freer access to information and the growing role of information in the system and its dependence on intellectuals and specialists. These, in turn, suffer constantly from the communications straightjacket and press for acceptance of modern public opinion and relaxation of controls. The prospects for these hopes are dim in the short term but the intelligentsia has good long-term prospects to win support from the leadership.

In this reviewer's opinion, ideas like "persistent pressure of unsolved problems," "a broad front of social changes" or "rising expectations of access to information" among the increasingly literate population, however vague, are nevertheless pointers for study and analysis. Small but constant changes in opinions, attitudes and beliefs, not only among philosophers and intellectuals, but among ordinary people, their religious feelings, national or nationalist tendencies among them, especially the ever-growing and spreading countercultures, of a type described by Sosin, are all powerful and steadily operating factors of change. They put a growing strain on the official ideology and empty it of substance and bite, penetrate the political upper strata (don't *they* know or like the forbidden songs, unorthodox paintings, new and fashionable scholarly languages?) and help to transform the mentalities of people and leaders alike. The widespread phenomena of ambivalent, "bilingual" attitudes (referred to by Hollander), exemplified by the tendency of people to move from the use of the official language in public to a different one in private, certainly applies not only to free wheeling intellectuals but also to experts and political officials. Bearing such phenomena in mind, and studying them more closely, the stages through which the recently suppressed dissent manifestations (as Barghoorn and Biddulph have shown) may be more significant for the future than is currently recognized. The whole process began by the cultural ferment in the early sixties, moved into "non-public" protest and then became public, openly political protest in the late sixties, complete with publications, memoranda, a diversity of political streams and publicity and pressures from abroad. The Soviet leadership became aware of this potential for spillover once the lid is opened too widely and for too long a time, and moved to halt the process. But nothing can censor or arrest the broader social and cultural changes, new ideas and ideologies spreading in ever-larger circles into new layers of the population. Hence more phenomena of ambivalence and dissatisfaction, and pressures for change in the system are unavoidable.

These cannot be stopped; changes are occurring and will go on. However, those changes do not automatically move into the direction of liberalization, guarantees for human rights and so forth. Feifer's Russian informers speak of more sinister prospects to be reckoned with. Friedgut's interviewees pointed to phenomena of chauvinism, racialism, plain obscurantism of different breeds; and the famous letter to the Politbureau written in 1970 by Sakharov, Medvedev and Turchin envisaged even "fascist" menaces in Soviet society. Hence, although changes such as even more dissent in the future are easy to forecast, the character of these changes is more difficult to anticipate. The real potential of the Soviet socio-political forces is veiled by the apparently self-satisfied and shallow ideological facade; they may seem menacing and unpredictable in the eyes of the rulers themselves, despite the piles of reports on the moods of the population pouring in from the political police. Maybe even because of the content of such reports!

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Expulsion of a Minority: Essays on Ugandan Asians. Edited by Michael Twaddle. (London: Athlone Press, published for the Institute of Commonwealth Studies, 1975. Pp. 240. Price not given.)

In 1972 the Ugandan government suddenly expelled virtually all Asians (that is people of Indian and Pakistani descent) on 90 days' notice. Both the policy and its implementation came as a complete surprise to observers and participants alike. This collection of original essays offers several corrections to the conventional explanation that the expulsion was the inevitable consequence of Asian economic exploitation of the African majority in a supposedly classic plural society.

Michael Twaddle summarizes the important corrections in his introduction. First, contrary to the attention they received from the world press, Asians in Uganda amounted to a surprisingly small percentage of overseas Indians and Pakistanis. Furthermore, in the aggregate, they were better off economically than many other Indian overseas communities. Second, the image of Asians in Uganda as shopkeepers who exploited Africans is overdrawn (by, among, others, African traders hoping to take the Asians' places), and ignores those Asians who may have constituted the majority of Asians, government employees and poor artisans. Third, the so-called "Asian community" was merely an abstraction imposed by outsiders and not shared by the many subgroups within the racial category. Ismailis, Goans and Hindus are discussed in separate chapters by Gardner Thompson (in an outstanding essay), Jessica Kuper and Rohit Barot, because they form separate communities practicing different religions and sharing little cooperation and less intermarriage. Within each of these groups there are factional rivalries of varying intensity which often prevent political unity.

But no matter how inaccurate conventional wisdom, stereotypes imposed by outsiders contributed to the expulsion decision. Dent Ocaya-Lakidi presents evidence of the anti-Asian stereotypes held by elite and by mass Ugandan Africans. The stereotype that struck the most sensitive nerve was the presumed failure of Asians to "integrate." As Sir Amar Maini notes, there were voices in the several Asian communities urging fuller identification with African aspirations, particularly during the nationalist struggle. Asians also actively participated in all three major Ugandan political parties. Certain Asian communities, most notably the Ismailis, opened their schools to African students and invested large sums in Ugandan development schemes. Both the present Aga Khan and his grandfather consistently urged their followers to do all they could to identify with indigenous concerns in their country of residence—including adoption of local citizenship. Yet, as Thompson reluctantly admits, even though the Ismailis were the most integration-minded Ugandan Asian community, their links were primarily financial and institutional and rarely social. African spokesmen, however, demanded that Asian women accept African husbands to "prove" that Asians were fully loyal to Uganda. The rejection of this demand—one on which Amin publicly insisted in a conference with Asian leaders in December 1971—provided all too easy a rationalization for the expulsion.

British Protectorate officials helped to create the racially defined social structure that tended to reify both Asians and Africans into monolithic separate categories. Ocaya-Lakidi points to the role Asians played as middlemen shielding the manipulation of British officials from African view. He overstates, however, the degree to which this was conscious British policy. Nizar Motani carefully traces government preference for Asians rather than Africans in the lower levels of the civil service during the preindependence period. In an excellent essay on the Ugandan economy Michael Tribe draws a more serious indictment in showing the shockingly high guaranteed profit margins legislated to protect Asian cotton ginners and sugar plantation owners at the expense of African growers and laborers.

Equally regrettable have been the remarkably inconsistent policy positions on citizenship rights of Ugandan Asians taken by the British government after Ugandan independence. In an able essay marked by understated irony, James S. Read demonstrates the British withdrawal of full citizenship from East African Asians who had earlier accepted British passports. Britain's entry into the European Economic Community permits nationals of other member states unrestricted entry while denying it to Asians holding United Kingdom passports and thus without claim on any other state. These shifts in British policy clarify the refusal of the Indian government to admit Asians not holding Indian passports. In any event, few East African Asians were prepared to accept the fall in living standards that moving to India would imply. The early history of India's relationship with Indians overseas is ably discussed by Hugh Tinker. The later attitude of the Indian government is elaborated by Anirudha Gupta.

The most puzzling feature of this collection of exceptionally competent articles is the total absence of any effort to explain how and why the Amin government decided upon expulsion. Asians had little political power, and as Jessica Kuper says, "the varient adaptations of the different Asian communities were in the end irrelevant" (p. 68). The expulsion was a political decision and by no means inevitable, as Twaddle asserts. Dennis Pain sheds new light on the Nubians, now a force to be reckoned with in Ugandan politics, but strains to develop similarities between Nubian and Asian traders when he could more profitably have sketched Nubian influence in official policy making. Tribe contents himself with a presentation of the "economic rationale" (p. 172) for the expulsion which is not the same as suggesting which economic factors Amin was likely to have taken seriously.

Given the casual nature of policy formation in Amin's government, a coherent account would be difficult to construct. However, Amin's desire to expel the Asians was evident to high level civil servants shortly after his *coup*. The government took a census of Asians on the basis of race in October 1971. Two months later Amin held a public conference with Asian leaders warning them to change their behavior. The following March the Israelis were expelled. Then in August came the first of the Asian expulsion decrees.

The internal instability of the Amin regime is a consequence of divisions in the army when Amin's faction took power. His government rapidly alienated most groups that originally regarded it with favor. It often changes policy radically in its search for new support. Most governments prefer short-run advantages to long-run benefits. How much more so must have Uganda's.

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Ideologies and Illusions: Revolutionary Thought from Herzen to Solzhenitsyn. By Adam B. Ulam. (Cambridge, Mass.: Harvard University Press, 1976. Pp. viii + 335. $15.00.)

It has always been a pleasure to read a new book by Adam Ulam, a scholar of solid erudition who has a finely honed mind, a refreshing wit, and an elegant writing style. And the dust jacket blurb promises "brilliantly lucid essays." What a disappointment therefore to have to review Ulam's new book.

The volume contains 17 pieces of varying length, most or all of them published previously within the last 12 years. So eager, indeed, was our author to publish another full-length book that he reprinted materials from several of his own previous books. The resulting collage does not constitute a coherent whole. There are three essays on Russian revolutionary ideology, stressing its specifically Russian character, its dangerous utopianism, and its relationship to Bolshevism. Six essays deal with Lenin, Trotsky, Stalin, and Tito, or with selected aspects of their ideas, talents, leadership styles, their achievements and failures. Seven relatively brief journalistic *pièces d'occasion* discuss ideological dilemmas of Soviet foreign policy. The book closes with a review of Solzhenitsyn's *Gulag Archipelago*.

On page 70, Ulam mocks the childishness and grandiose conceit of Marx and his followers for claiming to have discovered something which has eluded countless philosophers and historians. Perhaps. But anyone who writes a book must believe that she or he is offering *something* that has not been said before. Yet in this publication I have not discovered anything new. There is no new information, no re-ordering of what already is known, no new synthesis or new idea. The book is not systematic enough to serve as an introduction to students; it contains too many learned allusions to serve the informed lay reader. It does not seem to serve any discernible public.

This book, in fact, is a series of warning sermons about the dangers of utopian thinking in politics—these warnings directed to the younger generation, whom he insults by calling them children: "The greatest evil of our society," he quotes from the journal *Vekhi*, "is the spiritual rule by children," and he adds that this is still relevant today (p. 67). He implies that these infantile radicals are idlers spoiled by affluence: "Nothing is more productive of intellectual discontent than material progress," he writes (p. 49) and praises the "older and wiser" anti-utopianism which considers material progress irrelevant to human happiness (p. 52). Further, he warns against dangerous theories of history: Behind unsound theory there usually is some bad history, he repeats on pages 19 and 65; but that seems to me to be an empty quip—as if "good" history were a guarantee for good theory, or as if one could even separate the two. And, then, what is good history? Ulam does not tell us this, except by making clear that he is committed to a gentlemanly liberal conception of history and politics.

The first three essays lament the follies of Russian radicalism and seek to uncover the reasons for the triumph of Bolshevism: certain fatal psychological flaws in modern liberals such as Herzen; Plekhanov's responsibility for introducing Marxism to Russia; Martov's failure to develop a counter-Bolshevik Marxism; and again and again Lenin's uncanny pragmatism of power. I found the chapter on the three styles of Marxist leadership sensitive, perceptive, and suggestive—a fine essay. But much in these laments is fatuous; and even the interesting ideas are insufficiently developed. Foremost, the treatment is too ideological in that the ideas are inadequately related to the social, economic, and political facts of life in nineteenth-century Russia.

In the section dealing with Soviet foreign policy, the underlying themes are cold-war verities. Repeatedly Ulam refers to the delusion gripping the men in the Kremlin: for all their hard realism they are prisoners of an ideology which they cannot and will not give up. This rule of the dogma is, for him, the essence of Bolshevism (see pp. 189, 224). He compares this, at least by implication, to the fundamental reasonableness of the United States and therefore is compelled to tread carefully when it comes to the discussion of the ideological bases underlying American attitudes toward the Cold War. Contrasting Soviet anxieties over Titoist behavior with American nonchalance about polycentrism, he writes, "Few people in America lost any sleep over the fact that Britain and France are nuclear powers. . . ," forgetting how long Charles de Gaulle functioned as the Tito of the NATO bloc.

This exaggerated fascination with the ideology causes him to warm up ideas of Kennan that do not seem to have stood the test of time. The Soviet Union, he argues, requires a messianic ideology, a foreign threat, or an expansive foreign policy in order to survive: "With the loss of external dynamism, Russian Communism would find it difficult to cope with its own society" (p. 186). Further, he argues that with improvements in living standards, expectations for greater freedom will rise in the Communist world; and from this he concludes that "capitalism in our day cannot withstand long depressions. The whole rationale of Communism is in turn jeopardized by a long period of ascending prosperity" (p. 178). In fact, American capitalism has managed to survive an entire decade of depression (1930–1940); and the stability and rising prosperity of the German Democratic Republic and Bulgaria also gainsay his theory. Indeed, the Communist regimes that have been racked by revolt have been the economically inefficient ones. In the end of this section, Ulam urges the West to put its house in order. Our disunity will tempt Moscow into adventures and will lessen their willingness to ameliorate international tensions. The moral is that the USSR will soften if we regain our stability and political resolve. This Kennanism is no more convincing than the domino theory.

When decent, gentlemanly liberals lament the uncouth nature of politics in other countries, they might at least acknowledge the heavy responsibility which the West bears for the dilemmas of nationalism, westernization, and revolution. Lacking this acknowledgment, I find their homilies tiresome. The book is marred further by exaggerations, distortions, excessively bold speculations, and other tokens of scholarly irresponsibility, into which, alas, the pleasant essay style can seduce an author. Even Ulam's witty apercus fall flat when they serve only to support his ideologies and illusions.

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Political Socialization in Eastern Europe: A Comparative Framework. Edited by Ivan Volgyes. (New York: Praeger Publishers, 1975. Pp. 199. $16.50.)

This volume reflects a broadening range of concerns in the study of the political dynamics of European communist states. The weakening hold of the totalitarian "model" in the last decade has given rise to concerns with the diversity of inputs in socialist polities, with interest groups and their behavior, and other topics previously given rather short shrift. Political socialization is certainly among these—but it is also a "traditional" concern *within* the totalitarian perspective, which made much of regime command of education and media resources, etc., and their mobilization on behalf of socialization and re-socialization.

Political Socialization in Eastern Europe, then, should interest specialists in communist politics, whether of "behavioral" or more traditional persuasion, as well as nonspecialists concerned with what new regimes, initially quite weak in legitimacy, have made of the resources at their disposal in the course of a generation. Unlike the Soviet Union, Yugoslavia and even Albania, the communist regimes of Hungary, Czechoslovakia, Poland, the GDR and Romania were not products of domestic revolution, but imposed from without with the backing of the USSR, on countries whose national traditions ran from democratic to feudal-aristocratic, from moderately Russophile to Russophobe. In each, the "formal" socialization structures—party, youth organizations, schools, trade unions and the military—and the mass media have confronted the informal and often recalcitrant structures of the family, peer and friendship groups and the churches in a combination of attack and uneasy accommodation.

There have been diverse results; this volume is most valuable as a description of them. Though Volgyes' introduction presents a "model" of socialist political socialization, the book neither achieves, nor aims for, a high level of abstraction. This is quite appropriate, since a solid description of the often ill-perceived reality here may itself be a useful contribution to the base from which a general, multi-system model of political socialization may be developed.

The success story, if such a term applies, is the German Democratic Republic. Arthur Hanhardt's essay depicts a conformist population amid the semi-affluence of this best-developed of socialist states. The two are connected: political conformity and hard work produce tangible results. The GDR is an achievement-oriented society, offering excellent education to the qualified, and rewards for both ideological and technical accomplishment; with its own species of *Wirtschaftswunder,* it can afford to do so. Important also is the lack of a democratic tradition: since the unmourned death of the Weimar democracy, this part of Europe has lived under two successive tightly-centralized undemocratic regimes. Prosperity, apolitically (readily discernible evidence of conformist behavior) and the poster-art symbols of a workers' state readily coexist.

In another domestically "orthodox" state—Romania—the tasks of political socialization are nonetheless quite different in emphasis. As Trond Gilberg shows, the thrust of socialization here includes a heavy economic component. While the years since the "little cultural revolution" of 1971 have seen much rhetoric devoted to the importance of being "red," Romania also needs, and thus far lacks, an "expert" population. The Romanian post-peasant work force is a far cry from the stolid and disciplined workers of the GDR. Romania stresses "socialist morality"—close to a substitute for the Protestant Ethic in "its clear economic connotations . . . insofar as duty, discipline, and honesty are crucial factors in the struggle for modernization and efficiency in industry and agriculture" (p. 163).

Falling between these extremes, Poland, Hungary and Czechoslovakia have proven less orthodox politically, evidencing substantial shortfalls in political socialization, and varying regime accommodations. In Poland, as Joseph Fiszman indicates, a persistence of traditional behavior, anti-Russian nationalism and a combatant church generate cross-pressures which lower receptivity of political messages and, indeed, leave many of the "socializers" less than completely trustworthy themselves. Hungary shares with Poland a feudal-aristocratic tradition resistant to Soviet-style socialism: as Volgyes shows, the accommodation here reflects this fact as well as the limits and opportunities of the post-1956 period. Apoliticality is now licensed, virtually encouraged, in line with Kadar's dictum that "he who is not against us, is with us." Otto Ulc's essay on Czechoslovakia, while it downplays somewhat the significance of a democratic tradition unique in Eastern Europe, indicates the bankruptcy of legitimacy so convincingly demonstrated by the "Prague Spring" of 1968, and the distance which separates official declaration from private and unvoiced conviction in the period of "normalization" since the advent of the Husak regime.

The record, then, of the continuing attempts to create a "new citizen" is a mixed one, and well presented in this volume, if brutally summarized in this short review. One might, however, have wished for more scope in the exploration of two questions. First, is not much of the relative stability of these states (for, by world standards, they *are* stable) as readily attributable to a righteously-denied but nonetheless real secularization of life, rather than to successes in socialization? Hungary is portrayed this way—the others might have been also. The contemporary regimes often seem to seek *apoliticality,* rather than fervid commitment; the conformity produced by this goal is compensated for, albeit modestly, in rises in living standards. Stability is the critical desideratum—leaving the population to pursue the "quiet life" has not been reckoned too high a price to pay.

Why, then, the massive bureaucracies of socialization? This is the second question—and here bureaucratic interests and inertia may play as great a role as any goal commitment. Sinecures, careers, and pensions are tied up in these, and they persist as vital matters to *apparatchiks* modestly qualified at best for any *other* work. One might have hoped the contributors would have asked more pointedly whether the "socializers," who often point to the work yet to be done, are interested in *seeing* it completed, if such were possible, or whether they simply seek to insure themselves from redundancy, as the attentions of the Giereks, Kadars and Husaks are drawn to other concerns.

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L'imperialismo industriale italiano 1908–1915. Studio sul prefascismo. By Richard A. Webster. Translated by Mariangela Chiabrando. (Turin: Giulio Einaudi, 1974. Pp. 616. L. 14.000) and **Industrial Imperialism in Italy, 1908–1915.** (Berkeley: University of California Press, 1975. Pp. 392. $22.75.)

Commenting on the English-language edition of his book, the Italian version of which may be assumed to have been the manuscript in its original form, Richard Webster assures us that his thesis has not been distorted by abridgments. Comparison of the two texts confirms this. Moreover, the shorter version allows the sharply defined categories through which Webster analyzes fascism as industrial-imperialism to stand out more clearly than in the longer edition where explanatory commentary occasionally obscures Webster's very straightforward proposition: "Though the Fascist dictatorship did not materialize until nearly four years after the First World War, the turning point in Italy's fatal course seems to have been reached between 1911 and 1915. . . . These were the years that counted," Webster adds, concluding, "the Fascist coup d'état of October 1922 would not have been possible if the Chamber of Deputies had not already been completely discredited in May 1915. Once in power, the Fascist Government itself showed that it had few concrete programs that were not taken from the National-Liberal and Nationalist programs of 1911–1915. The long postwar crisis only delivered the coup de grace to the parliamentary system. In reality, the decisive years for Italy go back to 1911" (pp. 15, 16).

Webster's book would appear to be yet another example of "origins" historiography. Consequently, Webster must face the problem inherent in any work that would describe events still to take place in terms of phenomena drawn from an earlier period.

What distinguishes Webster's handling of the problem of historical origins is his determination to avoid that logical dilemma by adopting a simple descriptive technique, locating and examining the specimen "fascism" at its first moment, ignoring all question of any subsequent change either in its nature or of its milieu. While Webster does not declare this his method, it is implicit in the structure of his book as well as in the documentation used to demonstrate his belief that fascism existed a decade prior to its formal institutional appearance.

The essential characteristics of fascism which Webster identifies as early as 1911 are expansionistic industrial-imperialism seeking *"Lebensraum"* or "economic space." The theme recalls Fritz Fischer's *Griff nach der Weltmacht* (1961) of which Webster's book may be called a companion, especially in its emphasis on the 1914–1918 war as fundamental to our understanding of fascism. As Webster sees it, however, that war's significance for Italy does not lie in the course of the conflict, in the argument over war aims, or in the post-war crisis to which, he feels, historians have devoted too much attention. Rather the initial decision of May 1915 stands forth as the dominant historical fact. That step, and not any subsequent vicissitudes, was fascism already triumphant.

Dissenting from scholars who would link nineteenth-century overseas expansion to "totalitarianism," Webster distinguishes all such earlier forays outside the national frontier from genuinely fascistic imperialism. Italy's African campaigns, far from representing an embryonic stage of nascent fascism, were no more than the expression of vanity by the "ruling class" of an agrarian, scarcely united, pre-technological, hence "backward," society which tried foolishly to emulate the policies of its older, more advanced and far more potent neighbors.

A concept of "forwardness" and "backwardness" allows Webster to explain why Anglo-French expansionistic policies, which were first undertaken by states that had come to light side by side with "modernization," could be continued, even in the age of technological breakthrough, under regimes that were democratic or liberal, whereas the Italians (but also the Japanese and Germans) required fascism automatically once their industrial "threshold" was crossed. A simultaneity of "forward" and "backward" motions, if one may so epitomize the Webster model, called forth fascism as the only rule consistent with the needs of a technological world yet also able to manage the social-political turbulence let loose by the confluence of these contrary currents.

This conceptualization of the history of nations and their political behavior along lines of an asynchronic or synchronic historical mechanics is central to Webster's claim that 1911 was Italy's real "turning point," its industrial "take-off point." It was also in 1911 that the last residue of a "sentimental-agrarian imperialism," the Libyan War, was begun under the direction of the southerner Di San Giuliano, then Foreign Minister, from the region which becomes Webster's locus of the "backward" *Weltanschauung*. The "new men," the modernizing fascists, quickly turned away from the economically sterile African sphere toward the Balkans and the Levant. This was Italy's *"Drang nach Osten,"* an impulse which won de facto victory over the old regime in May 1915, the declaration of war confirming the demands of the industrial-imperialists written into the London Treaty.

Also in 1911, Webster reminds us, the Nationalist Association was formed from which fascism got whatever ideas it later put forth. The concrete significance of the Nationalists, as Webster sees them, was that they gave Italy in fascism its truly modernizing elements, dynamic individuals possessed of a highly specialized knowledge, who were in turn financially backed by industrial-financial "interest" or "pressure groups" based in central and northern Italy, i.e., the locus of "forwardness." Fascism, in Webster's opinion, was the *avant-garde* it always claimed to be, an intelligent capable minority, keenly aware of recent technological and military developments, justly contemptuous of an inefficient "Latin bourgeoisie," disgusted by the legacy of parliamentary corruption and of Giolitti, Italy's leading political personality whose historical function Webster defines as a clever manipulation of a "backward" society which enabled the governments of the "Giolittian Era" to conserve a semblance of statehood as the matrix in which fascism, the wave of the future, would erupt in May 1915.

By emphasizing fascism's innovating rather than nostalgic outlook Webster inadvertently helps lay to rest a colorful but wholly implausible array of "precursors," from Crispi and "Africanism" to *"sorelismo"* and "philosophic idealism." He also dissents implicitly from those who, as though mesmerized by the "German problem," have made Hitlerism a "model totalitarianism" of which the Italian tyranny was only an "incomplete" variety, therefore less important for our grasp of contemporary history. Webster is certainly correct in describing the London Treaty, which did ignore the African sphere while claiming territories elsewhere that were located far beyond the limits considered feasible by any previous Italian government, as blatantly imperialistic. Neither will many quarrel with his assessment of the intervention of May 1915 as a revolutionary act in defiance of an existing political majority.

But many readers will find it hard to contain the restless particulars of Italian political history within the static, rigid limits of Webster's categories. To contrast a "backward" or "Latin" bourgeoisie to a progressive "Anglo-Saxon" type; to idealize Southerners as revolutionaries, extreme conservatives or chauvinists, Northerners as consolidators or administrators; these and other generalizations subsidiary to the book's central conceptualization are too sentimental to satisfy the analytic rigor Webster hoped to bring to the problem of "origins" by means of a purely descriptive method.

As for that central "backward-forward" model or metaphor—for it has always been more allegorical than historical—is it not really the familiar vehicle for the political passions seeking some explanation of the "fatal course" of things by probing the anatomical flaws of the body politic? From Weber's anxious comments on German socio-political development written in the 1914–1918 war, or Marc Bloch's melancholy reflections on the "feudal-modern" ambivalence of France as it fell to the Nazis in the Second War, to the postwar notion of a "*verspätete* Nation" used to explain the "German catastrophe," it has long served as a rhetorical conceit of the historian in search of natural law, later becoming useful in academic and governmental studies of "nation building."

Among the Italian social scientists of the era Webster surveys, Gaetano Mosca, using the descriptive methods recommended by Taine, isolated the "backwardness" of his native South, for which he was criticized by another southerner, publicist of progress and democracy, Gaetano Salvemini. There particulars from Italy's political history raise the question of Webster's documentation, used to demonstrate that between 1915 and 1922 nothing essential to our knowledge of fascism really happened. To a remarkable degree, this book is based on the opinions of the Nationalists and other 1915 Interventionists, among which hotch-potch were to be found both Salvemini and Mosca. Those who wanted war were the first to share, or to say that they shared, what is still Webster's own evident contempt for the "backwardness" he finds in the Italy of the 1860–1915 era. By using these opinions indiscriminately, Webster lets his materials establish the categories he then uses to analyze Italian politics, making those same sources the proof of his contention that May 1915 was fascism and the liquidation of the "old regime."

The problem of origins is thereby resolved, but at the cost of a circularity of reasoning that is logically identical to the static view of things that has history stopping in 1915, making time future the same as time present. But for those who know what took place after the *dies irae* of May 1915, it will be evident that if many of Webster's heroes who, far from being technologically up-to-date were demanding a war which realistic "modernizers" such as Giolitti or Pareto viewed with alarm, did subsequently find their way with Mussolini into what developed as fascism, with equal certitude it may be said that the leaders of every faction of antifascism may also be found calling for war against "backward" Italy in 1915, as the careers of Albertini, Ferrero, Togliatti, Gramsci, Matteotti, Omodeo will establish, not to forget those two bitter rivals out of the "backward" South, Salvemini and Mosca. But that is another study, in which things are both themselves and their opposites, and in which nothing stands still.

JOHN THAYER

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Budgeting: A Comparative Theory of Budgetary Processes. By Aaron Wildavsky. (Boston: Little, Brown, 1975. Pp. vii + 432. $15.00.)

Over a decade ago Aaron Wildavsky published *The Politics of the Budgetary Process*, a book that soon became a seminal work in the field. Firmly establishing incrementalism as the paradigm of budgeting, Wildavsky laid the foundation for much of the subsequent research and analysis in the field. In *Budgeting*, Wildavsky takes on the more ambitious challenge of establishing a comparative framework applicable to a full range of budgetary processes. While he has partially succeeded, on balance the book falls short of any great expectations for another seminal work. While some new ideas and integrating concepts are introduced, the development of a truly comparative theory is hampered by rigid adherence to incremental assumptions and preconceptions.

In *Budgeting*, Wildavsky has drawn on much of his previous research and writing on the subject. The book is comparative in the best sense of the word; it examines budgetary processes in rich nations, poor nations, states, cities, a military installation, a water district, and even the small rural county of "Sugarland." Similarities in budgeting processes are most evident in the adoption of the roles of advocate and guardian. "The fundamental sameness of budgetary strategies around the world flows from the functional equivalence of budgetary processes. Everywhere there are those who want more than they can get, and others whose business it is to show them they can't have it" (pp. 203–04). Differences in budgetary processes are explained on the basis of two key variables, *wealth* and *predictability*. These concepts, in turn, are related to differences in political institutions, elite values, and size.

Beginning with domestic appropriations, Wildavsky reviews budgetary "experience." The early chapters focus on the rich and certain: U.S., France, Britain and Japan. An important addition to the earlier work of Davis, Dempster and Wildavsky is the classification of "shift points" in the linear regression model, examining the source of what were previously considered "random shocks" to the system. American cities are demonstrative of the poor and certain; "revenue" budgeting results from the severe constraints imposed by limited sources of funding. The American states are predictable, but fall between the rich and poor concerning wealth. Differences in state budgeting practices are explained on the basis of political structure. The poor and uncertain consist of developing nations where a turbulent financial environment makes the linkage between appropriation and expenditure highly tenuous. Examples are drawn from budgeting experiences in Indonesia, Chile, Argentina, Burma, Pakistan, the Philippines, Ghana and other modernizing nations.

From "experiences," the author moves to "comparisons." Clearly the best part of the book, section 3 examines basic similarities and differences in political and budgetary systems. Conflicts in budgeting are related to underlying political conflicts in the context of wealth, taxation, and allocation.

The final section deals with the question of reform. Wildavsky examines PPBS and zero-base budgeting in the Department of Agriculture, the political economy of efficiency, why PPBS fails, the Public Expenditure Survey Committee in Britain, and prospective reforms for poor countries and the U.S. Congress. New material is presented in his glimpse of "Potemkin Villages: Around the World with PPBS" which compares experiences with program budgeting. Consistent with his past work, Wildavsky remains an outspoken critic of such reforms.

Many readers will find *Budgeting* a convenient compilation of Wildavsky's previous work, but the book suffers from the inherent difficulty of weaving together often disparate material. The organizational net is overstretched in places. Although the author assures that every chapter "has been rethought, revised, and revamped to fit into a comparative framework" (p. xvi), a complete reworking and reorganization of material would have produced a smoother, more coherent result. The final section on reform consists primarily of articles that have appeared elsewhere; the new material is most predictable in arriving at highly critical conclusions. The continued attacks on the PPBS "straw man" would seem to serve no further purpose, and as Schick and others have observed, tend to obscure the more subtle changes in budgeting that have occurred as a result of reforms.

The major weakness of the book is not the inclusion of past work, but the compulsion to force new data into old molds. The past 12 years have seen a growing stream of research suggesting incrementalism of limited value in explaining the crucial questions of budgeting. Ignoring the many normative objections to incrementalism, studies by Wanat, Gist, Bailey and O'Connor, Moreland, Ripley, et al., and Natchez and Bupp, have raised analytical objections, suggestiong that incrementalism is true by definition, dwells on the obvious, and diverts attention from the interesting questions. There is a strain towards de-emphasizing variations and stressing the "basic sameness" in budgeting, whether they be agency strategies, the actions of reviewing bodies, or outcomes. A good example of preconceptions which structure conclusions is the comparison of rich and certain countries. The concept of proportionality, previously subsumed under incrementalism as "fair share," is now shown to vary significantly between developed countries. But instead of pursuing differences that suggest alternative norms and processes at work, readers are told that this is all simply more evidence of the institutionalization of incrementalism. On one hand, Japan is the "least incremental" of the four developed nations (p. 241) but at the same time Wildavsky claims they pursue "incrementalism with a vengeance" (p. 216) and "look incremental anyway you come at them, bottom up, top-down and seemingly sideways, too" (p. 217). As the term "incrementalism" applies to virtually every aspect of budgeting, conceptual confusion multiplies and it becomes meaningless for explanatory or even descriptive purposes.

In spite of these problems, *Budgeting* is essential reading for any serious student of the subject, both for the introduction of comparative data and as a collection of past work. The excellent bibliography is also useful. Although constrained by past interpretations, Wildavsky performs a service in providing a point of departure for the work of others. Whether subsequent research will expand, modify, or refute the explanations he has suggested, *Budgeting* is worthy of our attention.

LANCE T. LE LOUP

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Electoral Politics in an Emergent State: The Ceylon General Election of May 1970. By A. Jeyaratnam Wilson. (Perspectives on Development 3. New York: Cambridge University Press, 1975. Pp. 240. $21.00.)

Jeyaratnam Wilson's descriptive analysis of the 1970 election in his native Sri Lanka is the most detailed election study since this reviewer's report on the 1956 general election. It follows closely the Nuffield model developed for studying British elections, demonstrating once again the strengths and limitations of that model when applied outside Great Britain.

The first 30 pages provide a neat summary of the major groups in Ceylon's political life—geographical, communal, religious and economic—as well as a useful sketch of party history since independence. The author rightly stresses that unlike other post-colonial "new" states, Ceylon has had universal adult franchise since the early 1930s. While national politics in the early years were largely the affairs of the westernized elite in both Sinhalese and Tamil communities, successive elections have mobilized broader and deeper strata of the populace. In the 1970 election, for example, nearly 85 percent of the qualified electorate actually voted, a remarkable turnout.

After a careful–and spare–account of the pre-electoral economic and political setting produced by the "National Government" coalition under the UNP Prime Minister Senanayake and a convenient recounting of the preparations for administering the election, the study analyzes the candidates' background, selection and some detailed discussion of personalities in the more important constituencies. Wilson's data confirm for this reviewer the view that an individual's socioeconomic background tells little about his likely political affiliation or behavior, despite the amount of scholarly effort devoted in our profession to demonstrate the opposite.

The focus on the campaign itself contrasts the populist politics on economic issues in Sinhalese areas with the continued salience of the language issue in Tamil areas.

Like other Nuffield studies, the party manifestos and the issues as argued in the press and radio are examined in great detail. The rice ration, reduced by the government as much as a year before the election, was used by the opposition to embarrass the incumbents, as each opposition has been able to do in the past. Increased living costs, inadequate medical care, high unemployment and the terms of a major World Bank irrigation project–all by-products of a persistently near-stagnant economy with rapidly growing population–were all used against the government. Its performance in promoting more food output and the greater availability of consumer goods were not as effective in garnering votes.

Confirming Kearney's and Woodward's observations, the 1970 election showed even less success for "independents" than previously. Moreover, as compared to earlier elections, 1970 was marked as a contest between two extensive, multi-party coalitions or fronts. It would have helped readers to probe further beneath the surface if there had been a fuller discussion of the problems of coalition-building in a polity like Ceylon's. The Nuffield model is least helpful here, since party structures and internal party processes prior to selection of candidates and definition of issues are relatively straightforward in Great Britain. But in France or Italy, for example, putting together electoral coalitions is a complicated business, as it is in the United States when both "parties" must be shaped up for a national contest. What concessions did parties or leaders make in order to assemble their respective coalitions? What side-payments were promised individuals or parties when an ambitious and optimistic candidate agreed to step down in favor of another coalition candidate? How were agreements hammered out regarding what issues to stress? To be sure, such data are hard to come by; much of the activity takes place locally or privately. But the study would have been enriched thereby.

There are other insights useful for students of South Asian politics. Wilson reports briefly on the development of the PLF, the radical youth group that precipitated armed rebellion a year after the election. In an epilogue he notes that the very steps taken since then by the security forces in order to prevent a recurrence are increasingly resented. Adding to the government's difficulties has been the near-stagnant economy, the quadrupling of the cost of petroleum imports, the sluggish price of tea and the need to go deeper and deeper into debt abroad. A "non-aligned" foreign policy appears to have produced few real resources for the government of Ceylon. Wilson expects a further polarization of political forces, with a general movement of the center of gravity toward the left.

There are always additional subjects that might have been addressed to help the reader gain a deeper understanding. There is tantalizing little on how local constituencies are organized, on how funds are raised, how power brokers operate to generate support for a candidate. There is a good deal less on the political organizational aspects of each coalition's electoral effort. The issues seem to be taken at face value, rather than assessed for their tactical utility as seen by the protagonists in winning this or that particular group of voters. And one is not entirely clear on how a particular component of either coalition is linked into its supporting constituency.

Wilson, like the rest of us, has been impressed by Ceylon's capacity to sustain competitive, democratic elections, with a minimum of electoral intimidation and a high electoral turnout. He shares our concern that an inflexible, overly-specialized economy, increasingly controlled by a lethargic bureaucracy, does not diversify or expand to keep pace with a growing population. He remains hopeful that democratic political processes will persist, particularly since the populist and reformist policies of the government have overcome some of the privileges and exclusiveness of previous regimes. But like others of us, he believes the future of democratic processes to be uncertain.

As with Wilson's other books, there are many illustrative examples that can only be turned to by someone very familiar with his homeland. It is a pity that books in this otherwise excellent Cambridge University Press series are so awesomely expensive.

HOWARD WRIGGINS

Columbia University

Korean Politics in Transition. Edited by Edward Reynolds Wright. (Seattle and London: University of Washington Press, 1975. Pp. xii + 399. $12.50.)

"The essential purpose of this volume," according to the editor, is to provide an introductory basis for understanding [South] Korean politics. While each of the 11 "chapters" or articles can be read as a separate entity, it is "the editor's intention" that the various contributions constitute "parts of an integrated whole." Further, the editor hopes that "the basic insights" of the articles would "in no way become dated with subsequent political events" (p. xi).

It is quite evident that the editor, Edward Reynolds Wright, Executive Director of the Fulbright Commission in Korea and Lecturer in Political Science at Seoul National University, is masterful in his introduction to the volume (pp. 3–8), in his substantive article and in his integrative remarks preceding all five parts of the book: The Background; The Structure and Processes of Korean Government; Major Contributors to Korean Politics: Students and the Military; Major Policy Focuses in Contemporary Korean Politics: Foreign Affairs and Economic Development; and Interpretations of Korean Politics. Various articles were authored by eight active Korean scholars, including the current Korean Ambassador to the United States, and two other productive American scholars.

The editors and all contributors share a common experience of having studied in the United States; eight Ph.D.s in political science and a J.D., all from respectable universities. Thus it is not surprising that the volume abounds in references to such illuminati as Max Weber, David Easton, Gabriel A. Almond, and Lucian W. Pye. One or two articles appear to be compressed versions or parts of dissertations; e.g., a comprehensive survey by Young-Ho Lee of "The Politics of Democratic Experiment: 1948–1974" is documented with 112 footnotes containing many valuable bibliographical entries that may be useful to those beginning to study South Korea.

Another striking fact is that all eight Korean contributors are either residing in South Korea today or serving the government, while there are over 100 political scientists of the Korean origin in North America. While some books by these Korean scholars "abroad," including one by this reviewer, are frequently cited, none of these authors are contributors to this volume. Certain "conclusions" such as the following may be read in this light:

> Thus the normal mechanisms for political responsiveness and responsibility . . . are lacking in [South] Korea. In the meantime, the realization of democratic ideals . . . must depend on the benevolence of the political leaders in power (p. 33).

Under the circumstances, Bae-ho Hahn's cogent analyses, including the following, seem cautious but balanced:

> The prestige and popularity of President Park over the last several years has been greatly enhanced by his initiating some bold programs in the economic sphere. . . . It remains to be seen, however, whether the present political authority structure will eventually succeed in establishing more enduring, legally-based forms of legitimacy that will result in more voluntary compliance of the masses toward the regime (p. 306).

In view of the contributors' backgrounds, it is not surprising that most of them utilize many of the theoretical frameworks and quantitative methodologies prevalent in political science in the United States in the 1950s and 1960s. One clear departure from this apparent pattern is the exceptionally bold article by Pyong-choon Hahm, current Ambassador to the United States: "Toward a New Theory of Korean Politics: A Reexamination of Traditional Factors." Hahm declares that a Western or Japanese scholar "is simply requested to free himself of his culture biases" (p. 334) when studying Korean politics of the past or present. He deplores that

> there is a kind of conspiracy or obsession on the part of almost all of the theorists to establish power and wealth as the two values with which to evaluate every culture or civilization (p. 330).

He observes that these two values, when applied to Korean politics inexorably lead to the conclusion that Korean politics had "gone wrong" (p. 329). He believes that old Korea had accepted "clean poverty" (p. 346) and even "military weakness" as the price to be paid for "other goals it valued more highly" (p. 335). The quintessence of these goals, according to Hahm, was "humanness" (pp. 346–49). He then asserts that

> Koreans were prepared to appease aggressors or

> accept military defeat if the price for military viability of the nation was the dehumanization of the governmental process.... [Even] sanguinary purges [during the Yi dynasty] have been often confused by the students of Korean political history with factionalism among the literati.... [But] their factional divisions contributed to the preservation of humanness in the power process (pp. 350–51).

No doubt these sweeping assertions will be challenged by some, particularly by author(s) cited in the article.

Among the most carefully-written chapters are: "Students and Politics," a lucid article by Byung-hun Oh that reflects years of research; and "The Military and Politics in Postwar Korea" by John P. Lovell who skillfully traces the ever-growing role of the military in South Korean politics since the founding of the army that had become the fourth largest military establishment in the world by the end of 1956 (p. 168). Although Princeton Lyman's analysis of economic development in South Korea is one of the most readable, based on an earlier version published in the *Asian Survey* in July, 1966, no footnotes are given for the article in this volume.

The editor believed in his preface that the basic insights of the contributors should "in no way become dated" (p. xi); not much fresh data are found in this volume. Nevertheless, the volume succeeds in providing a "comprehensive introductory volume on contemporary Korean politics" (p. xii).

JOHN K. C. OH

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International Politics

The Burma Delta: Economic Development and Social Change on an Asian Rice Frontier, 1852–1941. By Michael Adas. (Madison: University of Wisconsin Press, 1974. Pp. 256. $13.50.)

In his book *British Rule in Burma, 1824–1942,* the late G. E. Harvey declared: "When we [i.e., the British] arrived in 1852 the delta was 'tall jungle and high grass, where the elephant dwelt and the tiger held dominion.' When we left in 1942 it had seven million acres under rice, an unending plain of wet ricefields larger than Wales.... Its colonization is an unrecorded epic" (p. 49). Now, some 30 years later, Michael Adas has not merely recorded that epic, but has subjected it to meticulous analysis as a case study of colonial policies and achievements in the era of "high imperialism." This task he has accomplished with a rare combination of insight, sympathy, and intellectual rigor, and the result is an outstandingly valuable contribution to Southeast Asian studies.

After a brief introduction the text falls into four parts of unequal length. Part I, "The Foundations of Social and Economic Development," comprises a single chapter which examines the character of the Delta, as an essentially underdeveloped region before the British annexation of Lower Burma in 1852. This is followed by the two most substantial parts of the book, namely "The Early Phase of Growth, 1852–1907," and "The Decades of Transition, 1908–30," which deal, respectively, with the expansive era of colonization and with the succeeding and increasingly problem-ridden period of the closing rice frontier. Thereafter Part IV examines the foreseeable—but by too many not foreseen—denouement, in a single terse and vivid chapter entitled "The Depression and Burma's Time of Troubles." Finally Adas has written a brief but masterly conclusion whose relevance extends far beyond the geographical limits of his case study, and has also appended an excellent discussion of the major statistical sources and an invaluable bibliography.

As is apparent from the outset, Adas is what may be called a constructive revisionist. Thus in his opening paragraph he offers an alternative to the commonly held view that the pre-1852 century of Kongbaung rule in the Delta was an era of suffering and strife. Arguing that such factors as disease, infant mortality, and the level of agricultural technology represented far more significant restraints than either the petty armies or the extortionate officials of the Kongbaung rulers, he suggests that the region should be seen as a frontier area on the southern periphery of a slowly expanding traditional society concentrated in the Burman heartland of the Dry Zone. And, he adds, "the events which led to the decline of the Delta as the nucleus of an independent Mon society and culture belong to a much earlier period" (p. 16).

With this general line of argument I, as a geographer, am basically in agreement, except that I do not think it goes far enough. For the more one studies the major deltas of Southeast Asia as a group, the more one becomes convinced that in none of them, prior to the introduction of nineteenth-century hydrotechnology, was a sufficient degree of environmental control achieved to make possible the support of more than a very low density of population. In short, the greater part of all these deltas had never been anything other than swamp and jungle before the nineteenth century. Such an explanation of the retarded development of the Burma delta seems to reinforce Adas' interpretation of the way in which the latter, once it had been controlled by modern means, quickly proved able to support a higher density of population at a higher standard of living than the Dry Zone had previously done. But thereafter, while opportunities expanded in the Delta, population expanded even more rapidly, as improved nutrition reduced the infant mortality rate, and increasing numbers of migrants, both from the Dry Zone and from even more congested areas in India proper, moved in.

In discussing the new pressures which developed as the colonizing phase was overtaken by the closing of the rice frontier, Dr. Adas may seem to be too generous in his judgments of British policy and also of the Chettiars who occupied an unenviable position at the center of the developing storm. Nevertheless I applaud his refutation of the charge that the majority of Chettiars were a hard-hearted lot of swindlers, cheats, and oppressors, no less enthusiastically than I relish the devastating deployment of his revisionist skills against the pretentious humbug that the central aim of all imperialist enterprises was to bring a people's products, but not the people themselves, into the world economy.

CHARLES A. FISHER

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The Standard-Vacuum Oil Company and United States East Asian Policy, 1933–1941. By Irvine H. Anderson, Jr. (Princeton: Princeton University Press, 1975. Pp. 260. $12.50.)

Anderson's study offers meticulous details of the intricacies of petroleum diplomacy in the prewar Far East and a rare insight into the struggle for mineral resources in Asia. Viewing oil as a basic source of conflict between the United States and Japan, the author closely examines the role played by the Standard-Vacuum Oil Company in the formulation of United States petroleum policy in Asia, at times even suggesting that the oil company dictated United States policy.

This historical experience carries a significance which transcends Japanese-American complications of the prewar era and has contemporary implications. The author never suggests that conflict over oil was the cause of the Pacific War, but an important factor that accelerated the pace toward the confrontation between the United States and Japan in East Asia. Even before oil assumed importance as fuel for naval vessels, tanks, and aircraft, the two countries had both been on an expansionist course in East Asia at least since the 1890s, which makes it hardly surprising that the two eventually came into conflict.

According to the dictates of international power politics, each tried to maintain influence in an area of the world of great potential economic and political importance. The interests of the two countries in East Asia were in sharp conflict, as Japan rigidly adhered to the position that she, as the most advanced industrial nation in Asia, ought to have dominance over her neighbors, a policy which found expression in the notion of a "Greater East Asia Co-Prosperity Sphere." Diametrically opposed was the American view that foreign interests in China should be governed by strict legalistic rules of international conduct which guaranteed Chinese territorial integrity and equality of commercial opportunity.

Within this context, conflict developed over petroleum which began to assume a major role not just in industry but also in the military sectors. This friction was bound to become exacerbated, because by the late 1880s Japan had launched an intense program of modernization under the Meiji Restoration, and was becoming increasingly dependent upon the United States for its oil needs. Moreover, petroleum trade in Asia proved to be very lucrative, making a monopoly of the East Asian market increasingly attractive.

Lured by these economic considerations, Japan began to seek alternate sources of oil in Southeast Asia and to drive American oil companies out of East Asian markets. Initially in Manchuria, the Japanese-controlled Manchukuo government in 1934 formed a petroleum monopoly which finally forced American oil companies to withdraw completely from that area. Ultimately of more significance, a 1934 Petroleum Industry Law in Japan established extensive control over the industry and required all distributors of petroleum products to build up and maintain a six-month stockpile at their own expense.

These developments, coupled with the American concern over the possible loss of other precious resources such as tin and rubber in Southeast Asia, entailed the direct involvement of the United States government to protect the interests of the American-owned companies. The meaning which Anderson attaches to this government involvement, however, deserves comment. He presents his discussion of the government's role in a way which seems to support the basic Marxist tenet that a capitalist nation's foreign policies are dictated primarily by economic considerations, thus failing to treat that as a normal diplomatic function to protect the interests of United States citizens and corporations abroad.

Nonetheless, it should not detract from the important contribution of this volume to existing scholarship about the diplomatic intricacies in East Asia prior to the Second World War. And the volume should be considered as a first-rate effort to treat conflicts between the two nations over petroleum as one, among many, causes of the war.

NACK YOUNG AN

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The Politics of Global Economic Relations. By David H. Blake and Robert S. Walters. (Englewood Cliffs, N.J.: Prentice-Hall, 1976. Pp. xii + 240. $7.50, paper.)

For anyone who has tried to teach a course which deals with IMF, IBRD, GATT, IADB, SDRs, NTBs, and UNCTAD not to mention tunnels, snakes, and dirty floats, this book is a real contribution. It is arranged according to a number of prominent issue-areas in the field of international political economy including trade, the monetary system, multinational corporations, relations between the rich and poor, and economic policy making in the United States. It delineates and summarizes the most important arguments associated with each of these topics.

The device which the authors have adopted for presenting their material is to analyze different questions in terms of a debate between liberal and radical positions. There is a fair statement of these two paradigms in the first chapter, and most of the arguments over specific issues can be stated in terms of a radical liberal debate. These range from whether foreign economic policy making in the United States is a product of corporate pressures and preferences, or a pluralist mishmash, to whether multinationals stifle or promote development in less-developed countries. Generally Blake and Walters are content simply to state the positions of the opposing sides without trying to decide between them, although occasionally they do make judgments of their own. When the liberal-radical split seems inappropriate, such as in their discussion of the costs and benefits for the United States of having the dollar as an international reserve currency, they wisely resort to the most prominent arguments on this particular issue. In sum, this book offers an excellent framework on which to hang a course on the political aspects of international economic relations.

However, in its even-handedness, prudent judgment, and selection of liberalism and radicalism as dominant analytic positions, *The Politics of Global Economic Relations* uncritically and explicitly accepts the common wisdom of recent years, namely that the world is growing more interdependent. This interdependence is not to be found in the choice of states, a form of interaction which was the stuff of conventional balance-of-power theory, but rather in the working of an autonomous global system—described not in terms of the relative strength of states, but in terms of a set of transactions involving national and transnational, public and private actors, over which individual governments have only limited control. Political leaders cannot accomplish their objectives through national means. "The problems confronting advanced states and developing countries are," Blake and Walters argue, "resolvable only by international action" (p. 234). It is society, either domestic or transnational, which determines the behavior of the state.

Given this assumption, it is not surprising that Blake and Walters have chosen liberalism and radicalism as the two themes around which they orient their discussion and have ignored a third position—mercantilism—which has received increasing attention in recent years. Mercantilism sees the international economic system as a reflection of state power; more specifically, the postwar global order is seen as a product of the power and policies of the United States. Interdependence is a result of this open system. Here the basic causal argument runs from the state to the society, while in both liberalism and radicalism it runs from the society to the state. The difference between the latter two is the importance that they attribute to different social actors: for radicalism, it is the dominant sector of the economy which ultimately determines government behavior, and for liberalism it is any group that can get it together. Since this general form of argument, in which the state is the dependent variable, has dominated analyses of domestic politics, it is not surprising that it should have finally come to international politics as well. However, there is at least a plausible case to be made for the application of conventional international relations theory (which views economic structure as a function of state power and policy) to global economic issues. It is unfortunate that the authors did not do more with this position, but it would have presented serious problems for the basic premise of the work, which is that the world is growing more interdependent.

The assumption of interdependence also leads to a particular kind of policy prescription, one which is close to at least the rhetoric of current American government positions. Blake and Walters argue: "But the United States must, during the 1970s, formulate a new set of foreign policies that are likely to result in even more constraints on America's freedom of action than its agreements in the 1940s" (p. 1). They also consistently maintain the necessity of recognizing "the legitimate concerns of the less-developed countries. Poor states feel that in present international economic and political relations their interests are being ignored. A stable order cannot be created as long as this is the case" (p. 222). While they recognize that implementing such policies will be difficult, they ignore an option which emerges from a mercantilist perspective. Namely, if the existing order cannot be maintained because the power and policies of the United States are changing, the most prudent course is to try to make the world less interdependent: to move toward a less open system because maintaining economic ties at existing levels can lead to political costs *which are far higher than the economic benefits* of a liberal order. Among industrial nations, developments such as the dissatisfaction with openness within the American labor movement, violent clashes over the import of Italian wines into France, and scandals involving the corruption of allied governments by American multinationals, suggest that there are high costs to eroding the sovereignty of individual states. In terms of closing the gap between the industrial and developing world, or even improving the per capita GNP of backward countries, it is not clear that the advanced nations can do much, or even that their domestic constituencies will allow them to do what they might. Under such circumstances, tensions will be more eased by controlling relationships than by pressing for even lower trade barriers and freer capital movements, or by making promises to less-developed areas or recognizing the legitimacy of their claims.

STEPHEN D. KRASNER

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The Hovering Giant: U.S. Responses to Revolutionary Change in Latin America. By Cole Blasier. (Pittsburgh, Pa.: University of Pittsburgh Press, 1976. Pp. xix + 315. $15.95.)

Near unanimity exists concerning the fundamental inanity of United States' Latin American policy, but a fairly lively debate goes on regarding its basic inspirations and motivations. Cole Blasier's study lends support to the traditional liberal interpretation which explains the entire messy business as informed by ill-conceived strategic scenarios acted out by policy makers infected with the fever of irrational anticommunism (or antinazism). Expansionistic capitalism managed by avaricious businessmen and seconded by diplomats cut from the same cloth is given its due, but Blasier fastens on the primacy of political considerations.

The major focus of the study describes and analyzes the United States' response to situations of rebellion, reform and revolution in several Latin American nations—Bolivia, Cuba, Guatemala and Mexico. Following a reflective and analytical chapter on "Explaining U.S. Responses (1910–1961)," the book concludes with an epilogue looking to more recent episodes in Chile, the Dominican Republic and Peru. The crucial question revolves about the differing responses flowing from disparate circumstances, influences and perceptions in tandem with more permanent principles guiding the formation of U.S. Latin American policy. The crux of the matter (and the analytical meat of the book) is set out in the "Explaining" chapter and even more succintly summarized in a series of explanatory "propositions" (pp. 236–38).

The book is fastidiously researched, carefully reasoned and basically well-organized. The research made splendid use of diplomatic archives and the book is studded with references to dispatches, letters, memoranda, and telegrams. It is also buttressed by material from interviews from some of the policy actors and their cohorts. All of this, finally, comes from a former career diplomat who assuredly knows the milieu and the nuances that he describes so faithfully. In much the same vein, the analysis is carefully drawn and cleanly argued, the historical chronologies of the several episodes are set out in detail, and the analytical context extrapolated from them is thoroughly explicated and balanced against possible competing analytical foci and conclusions. The 13 tables which appear throughout also assist in explaining the comparative data and explanations.

The book is also intelligently organized. The study describes the "stages" of rebellion, reform and revolution, juxtaposing and comparing the varying responses to the four Latin American countries. After focusing on a comparative analysis of the key problem of compensation for expropriated U.S. properties, Blasier then examines more closely the ultimate "reconciliation" with Bolivia and Mexico and the "paramilitary expeditions" launched against Cuba and Guatemala. All of that, of course, is designed to crystallize the comparative analytical context of the study and on the whole it succeeds rather well.

However, the book is not without its weaknesses. Not surprisingly, its very virtues tend to contain the seeds of its vices. The thorough research leads to the recitation of too much factual material that frequently brings the study close to the tradition of diplomatic history—a dreary genre, indeed. Historical descriptive data are certainly necessary to the analysis, but the study validly claims to do more than that, and too much descriptive material sometimes obfuscates rather than elucidates. Concomitantly, the author's successful attempts at weighing and sifting the alternative motivating factors involved in policy decisions results in some redundancy. (Perhaps this is intrinsic to the study's comparative organizational context, certainly a splendid feature of the book.)

In a more formal way, moreover, the purpose of the epilogue is generally clear and that chapter is assuredly germane, but its significance for and connection to the rest of the study is not adequately crystallized. Finally and most specifically, the rather longish sections given over to Mexico and Germany (pp. 105–16 and 124–27) seem out of proportion to their importance in the overall study and may well reflect the author's particular research interests more than their significance in understanding U.S. responses to revolutionary change in Latin America.

Despite these problems, Blasier's book features a splendid comparative analysis of the United States' Latin American policy. It is "must" reading for all students of interamerican and Latin American politics and highly recommended for those interested in foreign policy and comparative revolutionary change.

EDWARD J. WILLIAMS

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Britain and the People's Republic of China, 1949–1974. By Robert Boardman. (New York: Barnes and Noble, 1976. Pp. 210. $23.50.)

More than half of *Britain and the People's Republic of China, 1949–1974* is concerned with the early 1950s. The remainder is a far more sketchy coverage of the succeeding 20 years. However, these proportions are probably appropriate. In 1949 Britain thought of herself as, and was considered by others to be, a great power. In 1950 Prime Minister Attlee was able to play a crucial role in the dismissal of General McArthur after he and President Truman had opened up the possibility of extending the Korean War into China and using atomic bombs. In that year Britain's recognition of the People's Republic was an issue of major international importance; however, in 1972, when ambassadors were finally exchanged, no one noticed. Interestingly, the Chinese appear to have retained their perception of Britain as a world power rather late. For instance, they continued—at some cost—to use sterling for trade with the West until the 1970s. Yet the effect of Britain's decline has made the issue of Sino-British relations today of fairly marginal importance.

Robert Boardman's book is written almost exclusively from the British side. Little attempt is made to describe, let alone explain, Chinese attitudes and behavior. However, his analysis of the former is sometimes very good. For instance, his handling of the complexities of Anglo-American understandings and misunderstandings on the question of recognition of China in 1949 is excellent. The one major lacuna here is his failure to go into any detail about the *Amethyst* incident. Even though Evan Luard has covered most of its aspects in his *Britain and China* (Baltimore: The Johns Hopkins Press, 1962), it is important to treat the incident, which was significant, both for its effect on British politics, and as an indicator of the persistence of imperial attitudes. In April 1949 at the height of the civil war, HMS *Amethyst* was ordered over a hundred miles up the Yangtze. This was done without informing, let alone asking for permission, either the Communists who controlled much of the north bank, or the Nationalists on the south. Outrage that a British ship should be shot at or even be pinned down by fire, was whipped up by the conservative press and some observers have dated the beginning of popular cold war feeling in Britain to this period. The Chinese reaction to the incident—which Boardman does report—was to confirm their image of Britain as shamelessly imperialist.

Britain and the People's Republic of China presents problems for readers unfamiliar with British politics in the 1950s. Who for instance is the C. E. Davis whose attacks on SEATO are reported on page 74? There are also difficulties with the references to Woodrow Wyatt and Desmond Donnelly. They are correctly labeled as Labour MPs, but Boardman fails to point out that they were on the extreme right wing of the party which has an extraordinarily wide political spectrum. In fact the right wing of the Labour party has always been more ideologically anticommunist than the Conservative party. Boardman brings out well the contrast between the pragmatic outlook of the China traders, the Foreign Office, and most Conservative MPs on the one hand and the more doctrinaire attitudes of American and even Australian politicians and officials on the other. Policies on China have tended to demonstrate the paradox that Anglo-American relations in general are closer under Labour governments than under Conservative ones. During the 1950s, one major factor in this was the Conservative attempt to preserve British spheres of influence from American domination, which in extreme instances led to open clashes over Suez in 1956, but in general merely impeded harmony. On the other side American support provided much-needed legitimacy for Labour governments isolated in the prevailing conservatism—with a lower case and capital C—of the civil service and other institutions. However, equally if not more important is the real congruence of opinions on communism between American politicians and the right wing of the Labour party, members of which usually control the cabinet and are always picked to be foreign secretary. No American leader could have been more dogmatic and rigidly anticommunist than Wilson's foreign secretaries, Michael Stewart and George Brown. They are true believers, who needed no coercion or urging to stand by the United States through thick and thin on the issue of Vietnam or for that matter, on China. Boardman describes the Labour government's position on the PRC's membership of the United Nations during the 1960s according to which (as Messrs. Stewart and Brown frequently pointed out to Labour audiences) Britain consistently voted for the PRC's admission. What they did not point out was that Britain equally consistently voted that the matter be considered an "important" one, requiring a two-thirds majority, effectively blocking the PRC's entry, which explains the U.S. failure to protest the pro-China vote. Boardman's acceptance of the Foreign Office explanation that Britain changed its vote on the "important" question issue in the autumn of 1971 because China had won a simple majority the previous year, lacks plausibility. It is far more likely that the change was brought about by Kissinger's contacts with China and the announcement of the Nixon visit and that yet again Britain was merely following U.S. policy. *Britain and the People's Republic of China* is a good treatment of a sad story.

MARTIN G. BERNAL

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The Foreign Relations of Iran: A Developing State in a Zone of Great Power Conflict. By Shahram Chubin and Sepehr Zabih. (Berkeley: University of California Press, 1974. Pp. xiii + 362. Price not listed.)

This book is a welcome addition to the very limited descriptive literature on Iran. Its publication is now all the more significant as the foreign policy of Iran—especially in the newly strategic area of the Persian Gulf—poses a quandary for the United States. The recent revelation that former President Nixon promised, in 1972, to sell the Shah any non-nuclear American weapons system (not mentioned here) as well as growing concern with the sales policies of American armaments manufacturers further enhances the importance of our understanding precisely what the Shah of Iran is up to. Chubin and Zabih do not answer that question but a close reading of their book helps.

Their work is a study of the "content and outcome" of Iranian foreign policy and not of the perceptions or motivations of Iranian decision makers (read: the Shah) or his decision-making process. While they thank three key Iranian officials in their preface for "frank and comprehensive interviews" and mention Chubin's work as an "adviser" to the Iranian U.N. mission, the book seems to be based entirely on a close reading of the Western and controlled Iranian press and the published interviews of the Shah. Only two primary sources are alluded to in the text—an interview with Walt Rostow at the University of Texas and a lecture by Talcott Seelye of the U.S. Department of State at Columbia. Given what we now know of the fabrications and outright lies of American presidents—all so faithfully reported by the American media—one can only wonder at Chubin and Zabih's assumption that they are able to illuminate through such sources even the "content and outcome" of Iranian foreign policy.

Another assumption on which their work rests is that the Shah "makes every major foreign policy decision and most of the minor ones" (p. 10). The authors imply that only the constraints of limited material means and trained manpower impede the realization of the Shah's personal ambitions. They claim that there are no "formal" policy-making processes, no persons with influence on foreign policy who can claim independent power bases, nor "interest groups, lobbies, associational groups, or mass media that influence the conduct and content of foreign policy" (p. 10). But this view of Iranian politics is too limited. While data are nonexistent, it seems certain that the military plays a key role in at least indirectly influencing the Shah's thinking. It is also difficult to understand how SAVAK (the Iranian CIA-FBI), and other sources of royal intelligence could not have developed institutional interests which affect the ways in which their intelligence is structured and presented to him.

Moreover, the authors make clear the extent to which the Shah uses his conduct of foreign policy to augment his domestic political position by reflecting, to some extent, the basic orientations of the Iranian people toward foreign policy. Those orientations are basically two: "a sentiment nearly approximating a mystical belief in the efficacy of a policy of neutrality has long existed in Iran" (p. 62) and "a hardline oil policy is a *sine qua non* for any popular leader in Iran" (p. 273). Thus while the Shah does make all the major foreign policy decisions, it is well to remember that he operates within a context of considerable institutional and political constraints. These do not determine his decisions, but they certainly influence them.

One of the difficulties with the personal process of policy making in Iran, which is suggested by the authors, is that no institutionalized feedback mechanisms exist (p. 304). As a result, insofar as the Shah learns from his past policies, Iran's foreign policy will be strengthened. But the authors also show us that more often than not Iranian foreign policy successes seem to have been achieved by passivity, inadvertence, and confusion—benefiting from the turns of events and the mistakes of others rather than from a carefully designed, prepared, and executed policy. In discussing Iranian policy vis-à-vis the Soviet Union (p. 61), the Arab world (p. 150), Iraq (pp. 171, 192), and the Persian Gulf (pp. 213, 238, 240), Chubin and Zabih make clear how Iran, i.e., the Shah, has benefited, occasionally by taking advantage of opportunities, more often by the unfolding of events ultimately in the interest of the state. But the authors do not demonstrate that any learning process, other than the increasingly more vigorous pursuit of Iranian interests, as defined by the Shah, has been experienced.

What then are the implications of all this for the United States, especially for the American policy of unlimited arms sales? It should give pause to remember that insofar as Iranian public opinion is relevant to Iranian foreign policy, that opinion supports a policy of neutrality and of vigorous defense of Iran's oil interests. Personalized decision making hinders the development of sophistication in the making of Iranian foreign policy. The Shah may well see his wisdom rather than events as responsible for Iran's successes.

But what principles and goals seem to move the Shah? The authors make clear that the Shah has repeatedly disavowed an expansionist role for Iran, especially in the region of his chief concern, the Persian Gulf. But as long ago as 1958, the Shah stated that "Iran's supremacy over the Persian Gulf is a natural thing" (a September, 1958, press conference, p. 196). He sees the principal threat to Iran's supremacy, as he said a decade later, coming from "weak governments, weak countries, corrupt countries, where the elements of subversion will have free grounds for their activities" (an October 26, 1969 press conference, p. 242). He also believes that the "West is an unreliable ally in regional disputes" (p. 244).

A more direct clue to his beliefs about Iran's role came at the end of 1971 when Iran occupied the islands of Abu Musa and the Greater and Lesser Tumbs, in the Gulf, to prevent "their acquiring a 'nuisance value' in the hands of the enemies of regional stability" (p. 223). That military operation was executed with weapons which Iran had been acquiring for the four years preceding the 1968 British announcement of its intent to withdraw its forces from the Gulf (p. 215). In short, the Shah has staked out a region of overriding national interest, has acquired the physical means to advance that interest and has demonstrated a willingness to use those means. But how broadly he defines that geographical region and the conditions under which he will advance those interests and even a more precise definition of the nature of those interests remain unknown.

But with Iran's rapid acquisition of new weapons, one of the authors' principal generalizations seems especially apt: ". . . in its relations with the superpowers, Iran relies on diplomacy; but in its regional and local relations, where it is relatively strong, it prefers to emphasize might" (p. 303). The extent to which the United States should contribute to this emphasis remains a central question for American foreign policy.

MARVIN ZONIS

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The Question of Imperialism: The Political Economy of Dominance and Dependence. By Benjamin J. Cohen. (New York: Basic Books, 1973. Pp. 280. $8.95.)

The Question of Imperialism is one of several recent books which represents the emerging self-awareness of North Americans regarding their involvement in such issues as dominance, dependence, exploitation, and global inequality. Unlike many Marxist tracts, which assume the linkage between capitalism and imperialism and confine themselves to finding new fields of application (e.g., the European community as a new imperialist force), this book goes back to the fundamental questions: what is imperialism and how may it be explained?

The first question is definitional and is disposed of intelligently in the first chapter. Since imperialism is often defined so as to logically entail important causal antecedents or consequences (imperialism is the monopoly stage of capitalism), Cohen's first task is to decouple these properties from imperialism per se. Chapter 2, "Classical Imperialism," is the best in the book for here Cohen most thoroughly utilizes his solid economic training to analyze ideas presented in the literature. It is illuminating to see the translation of Marxian ideas into the idiom and conceptual machinery of modern economic theory. While the validity of the translation may be controversial, if one accepts it, one makes powerful gains in one's economic reasoning. Chapters 4 and 5, concerning the "View From the Metropolis," and "View from the Periphery," were disappointing. Chapter 4 restricts itself to an examination of the views of Baran, Sweezy and Magdoff, while excluding such contributors as Barratt-Brown, Emmanuel, and Semmel. Chapter 5, purporting to provide an analysis from the perspective of the periphery, neglects to mention such theorists as Cardoso, Sunkel, or Furtado. These are not merely lost footnotes; they are lost lines of argument. Cardoso, as the foremost dependencia theorist, has made pioneering contributions to our understanding of the domestic social formations that accompany external dependence, as well as to the structuralist interpretation of "restricted choice" and "distorted development."

Cohen's approach, broadly speaking, is to reformulate the Marxian theory of imperialism into rigorously testable propositions and to subject these propositions to logical and empirical scrutiny. As a trained political economist sensitive to power and market considerations, he has impressive credentials for such a Herculean effort. Cohen's economic training is apparent in several ways, most notably in the tendency to evaluate arguments logically (in terms of consistency, deduction) as well as empirically. It also is apparent in the translation of Marxian ideas into the paradigm of liberal economic thought. Though Kuhnians will be skeptical of the stability of meaning in cross-paradigm translations and theological Marxists will resist efforts to tamper with scripture, the approach is commendable. However, there are imperfections in its implementation.

Let me focus on two of them. The first problem concerns the approach taken toward evaluation of the empirical evidence. This approach involves providing examples for or against the theoretical argument. But in a natural historical environment where theoretical alternatives and confounding factors are not neutralized, it is impossible to draw disciplined conclusions. Numerous examples can always be found to refute or confirm the theory. Furthermore, since the statistical mode of evaluation was not selected, the verificational criteria shifted to a "necessary and sufficient" criterion (i.e., "if and only if capitalism is present will imperialism be present" rather than statistic's implicit yardstick, "sometimes capitalism, sometimes imperialism"). The proposition that capitalism is a cause of imperialism, but not the only one, suffers from this approach.

The second criticism concerns the major substantive conclusion of the book, that capitalism is not the cause of imperialism; rather the answer must be found in the anarchic nature of the nation-state system and the national security dilemma which it spawns. This conclusion is not merely wrong; the causal categories used are not complex enough for the problem. Political and economic forces can't be thought of as separate causal candidates to be lined up against the dependent variable for empirical evaluation. The separation is much too neat—not only may both be involved in an additive way; one may work through or in conjunction with the other. Unfortunately, these complexities are not explored, let alone modeled. There are no levels of causality here, no hierarchies of control, no structuring of constraints as well as simple cause-effect relations, no deep driving forces, background conditions, catalysts, mechanisms, permissive and final causes. The simple cause-effect methodology employed drains the Marxian interpretation of much of its power and obscures an adequate analysis of relations between the public and private sectors. The private sector may find itself opposed to statist interests as in the opposition of British free traders to Palmerston's colonial policy; or autonomous as in the early expansion of the multinational enterprises; or confusion may reign as in the uncertain mix of motives and consequences characterizing American labor unions, governmental foreign policy decisionmakers, and private corporations as they grope for a policy for the regulation of multinationals.

The choice of an economic or a political interpretation sets up a false dichotomy because it assumes both sets of variables operate at the same level of causality. But political factors are concerned often with setting constraints or parameters within which other causal relations may operate. The thrust of the "free trade imperialism" argument, as I see it, is that so long as a liberal, cosmopolitan orientation toward foreign countries results in a net profit for a society, a political role for the state is not necessary. This approach accommodates both economic interpretations as well as more direct forms of political rule and it does so without turning imperialist theory into vacuous assertions.

Developments in the bureaucratic politics literature demonstrate how difficult it is to talk of a "state's interest" even when it comes to security while the transnational relations literature ascribes a partial autonomy to private actors in the global system. Perhaps it will be years before the full weight of these approaches is appreciated. When this occurs, we may be able to provide a more adequate and sympathetic (but not necessarily more favorable) treatment of imperialism. For now, I remain unconvinced that the whole answer is to be found in an uncertain mix of *animus dominandi* and the national security dilemma.

JAMES A. CAPORASO

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The Making of a Missile Crisis: October 1962. By Herbert S. Dinerstein. (Baltimore: Johns Hopkins University Press, 1976. Pp. 302. $14.95.)

In the short span of 14 years a library of books and articles has been written to explain the Cuban missile crisis. Most attention has been paid to the United States because documentation on its policy is more complete, the least to Cuba because of the general assumption that the heart of the crisis was a conflict between the superpowers. Herbert Dinerstein seeks to redress this imbalance by examining the interactions of all three parties with particular emphasis on the Soviet Union and Cuba. His study relies heavily on textual analyses of the press, government statements, and specialized publications of each country to determine the perceptions and assumptions held by the policy-making elites. The author concludes that all three parties developed policy on the basis of contrasting assumptions, misperceptions, and exaggerated fears, resulting in a confrontation that neither Kennedy nor Khrushchev wanted.

Dinerstein traces the origin of the 1962 crisis to those factors which led to Castro's revolution, his conversion to communism, and to Khrushchev's acceptance of Cuba into the Soviet bloc. Thus, he begins with the Guatemala crisis in 1954. United States complicity in the revolution against the Arbenz regime convinced the non-Communist Left (such as Castro) that America was an inveterate enemy of reform and that the Communists could not be expected to bring about revolution. After coming to power Castro carefully avoided identifying with Cuban communism so as not to give the United States a pretext for intervention. Dinerstein argues that Castro's acceptance of communism was in large part a result of American (and Cuban) opposition to his reforms, particularly the Agrarian Reform Law of May 1959.

Soviet policy toward Castro during 1959–60 was hesitant and cautious. Khrushchev was uncertain which way Castro was going and how far the United States would let him go. The event, according to Dinerstein, which spurred Khrushchev to wholehearted support of Castro was the U-2 incident in May 1960, though throughout much of 1960 and 1961 the two leaders groped for a mutually satisfactory definition of their relationship both in ideological and practical terms. Dinerstein argues that the Bay of Pigs crisis played a significant role in leading up to the missile crisis a year and a half later. The failure of Kennedy to follow through the original attack with support from the marines apparently confirmed for Khrushchev the belief that the United States would not risk a nuclear war to roll back communism. Clearly Khrushchev made a grievous miscalculation in 1962 in assuming that Kennedy would not employ force to dislodge Soviet installations in Cuba.

The central mystery of the crisis, of course, is why the Soviet Union installed ground-to-ground missiles in Cuba in the first place. Was it to alter the unfavorable strategic environment in which the Soviet Union found itself (Arnold Horelick and Myron Rush); to reduce American credibility by exposing an irresolute United States (Arthur Schlesinger, Jr.); to force the

Allies out of Berlin (Michel Tatu); to obtain a German peace treaty and the denuclearization of China (Adam Ulam); or to protect Cuba (Nikita Khrushchev)? We will probably remain stymied on this question until the Soviets open their archives. Dinerstein offers no definitive answer here. He assumes that Soviet objectives were multiple and suggests that as many as three different strategies may have been contemplated within the Kremlin. These strategies are labeled the "golden bridge" (compelling the United States to accept a *fait accompli* but offering it a face-saving out); "bargaining-counter" (missile removal to be exchanged for a *quid pro quo* such as recognition of Cuba); and "brinksmanship" (taking a strong stand and holding firm). By means of an exhaustive thematic analysis of the official Soviet government statement of 23 October 1962 and three editorials on that statement Dinerstein demonstrates rather conclusively that the Soviet leadership was divided on how to respond to the challenge of the American naval quarantine of Cuba. His analysis essentially confirms what others (Roman Kolkowicz for one) have suggested about cleavages between the political and military elites during the crisis.

The author's conclusions constitute an indictment of all three governments for basing policy on illusions, misperceptions, and just plain ignorance. The United States, for example, failed to understand the depth and popularity of the Cuban revolution or to realize Khrushchev's determination to redress the strategic imbalance. Khrushchev believed Kennedy to be craven and the United States unwilling to risk nuclear war. He and Castro assumed that the Cuban revolution could be replicated throughout Latin America, and Castro counted upon Russia's nuclear deterrent to guarantee American inaction in the Caribbean.

Not all of the author's conclusions and foreign policy prescriptions will be universally accepted. His judgments that the overthrow of the pro-Communist government in Guatemala in 1954 was a "strategic defeat" for the United States and that the outcome of the Cuban missile crisis was neither a victory nor a defeat for either side are provocative and do not necessarily follow from the empirical evidence introduced in this volume. His argument that the United States should eschew intervention to keep communist movements from coming to power is plausible today but too general and blanket a prescription. But overall this study is a bold and imaginative work of scholarship which makes an important contribution to the literature on the Cuban missile crisis and to the study of foreign policy in general.

Though familiar with the typology developed by Graham Allison in *The Essence of Decision* Dinerstein makes no explicit use of his models. This analysis is largely a combination of the "rational actor" and "governmental politics" models with an occasional reference to issues relevant to the "organizational process" model.

JOSEPH L. NOGEE

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The New Political Economy of the Pacific. Edited by Bernard K. Gordon and Kenneth J. Rothwell. (Cambridge, Mass.: Ballinger Publishing Company, 1975. Pp. xii + 177. $13.50.)

This study is a volume of symposium essays originally presented in April 1973 at the New England Center for Continuing Education. Unfortunately, their utility is diminished because they have not been updated to take into account the dramatic effects on the political economy of the Asia-Pacific region brought about by the massive increase in petroleum prices and general international "stagflation" attendant upon the Yom Kippur War of that year.

As in most edited conference collections, the selections in the Gordon/Rothwell book vary in quality. Insofar as a unifying theme exists, it is the evolution of Japanese-American relations in the post-World War II period and the manner in which that relationship has affected other actors in Asia (and to a smaller extent Pacific Latin America). The essays are divided into four topical areas—strategy, economics, politics, and 20th century history. The historical section is least edifying, although Louis Morton's brief comments at the end of the section are more illuminating about how and why the United States and Japan went to war with one another than the two substantive chapters preceding his remarks.

Ericson, Iriye, Gordon, and Fukui address themselves to strategic problems in the Pacific, particularly the future of the Japanese-American Mutual Security Treaty and the role of U.S. forces in East Asia. Ericson worries about Washington's ability to retain U.S. bases in Japan in the face of growing nationalist sentiment but does not consider alternative locations in the mid-Pacific nor the kinds of military threats Japanese bases are supposed to deter. He maintains that U.S. bases in Japan are "universally desired" (p. 19) by all East Asian states but ignores what to this reviewer is instead a *temporary* Chinese reliance on American forces until Peking can balance the Soviet presence on its own. Gordon is taken with Lee Kwan-yew's 1973 proposal—never again raised seriously, to my knowledge—to create a multilateral noncommunist Pacific naval agreement as a means of sustaining an American military presence. But how such a force would be organized and against which contingencies it would be directed are questions left unexplored. Professor Fukui raises a basic set of questions on the feasibility of Japanese defense which deserve a book in and of themselves:

> If Japan was basically indefensible, due to her special geographical and physical characteristics, how could she be defended by the United States? If, on the other hand, the United States was capable of defending Japan, presumably because of her superior military capability, why could not Japan acquire her own military capability... ? If the external threat to Japan's security really did not exist, why should she be protected by the United States (p. 156)?

In the realm of economic policy, Gordon and Rothwell construct an excellent agenda of research questions ranging from differing national rates of economic growth and uncoordinated trade policies through uncertainty in foreign investment flows and economic nationalism (p. 6). But the worth of Sawaki's solid chapter on Japan's political economy is reduced because it does not go beyond 1973 into the effect of Japan's lower labor productivity resulting from massive wage increases to meet inflation after 1974 on its trade capabilities. Professors Rothwell and English provide useful brief chapters on Pacific basin trade and investment.

Finally, Ericson, Barnett, and Fukui purvey some insightful analyses of domestic Japanese politics, particularly the new appeal of the Japan Communist Party, the links between business and politics (currently being explored in the world press through the Lockheed scandal), and the complex relations within and between governmental agencies in the formation of public policy.

SHELDON W. SIMON

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The Arms Trade and International Systems. By Robert E. Harkavy. (Cambridge, Mass.: Ballinger Publishing Company, 1975. Pp. 291. $15.00.)

Robert Harkavy offers an important contribution to a growing body of literature on a very significant process: weapons transfer between nations. Harkavy's diversified career as an academician, investment analyst, and currently as public official (U.S. Arms Control and Disarmament Agency) is clearly reflected in his volume.

The book focuses on systematic comparison between the weapons markets of the interwar (1919–1939) and the postwar (after 1945) eras. The attempt to develop an orderly set of generalizations about a subject which has rarely received more than scant attention in the scholarly literature is, unquestionably, the most important contribution of the book. Harkavy, however, is carried away with his subject (as so many authors are) when asserting that the trade in arms "has become the weightiest and most important instrument of international power and diplomacy" (p. 1). This argument, and others like it, should have been tested as hypotheses, not accepted as assumptions.

Nevertheless, the volume is an impressive attempt to deal empirically with an inherently difficult topic. Though Harkavy did not collect original data on the arms trade in the postwar period (many others did), he is the only one who did so for the interwar era, searching through the files of the Military Intelligence Division of the War Department and similar archives. Furthermore, the author tries in this book to link the empirical material of his investigation with more theoretical discussions in the field of international relations. He emphasizes especially the relevance of systems analysis, and he actually offers a systemic model of arms supply in which the phenomenon under study is linked to various system characteristics.

Harkavy's methodological approach is rather cautious, as the difficulties of the subject require: "a tight delineation of cause and effect would be difficult to achieve," he asserts (p. 47). Yet it seems that at least some variables and relationships could have been dealt with by using more sophisticated or appropriate methodological tools. For example, while estimating the correlation between GNP and market shares in various weapons categories, the author uses the common statistic of Pearson's *r*, despite the fact that his sample is very small (N=12) and would, therefore, require a non-parametric coefficient.

Some of the substantive findings are very interesting. Harkavy discovers that revisionist powers, in both periods, tend gradually to take over arms markets traditionally controlled by status-quo powers (p. 99). Despite the fact that many will question the classification of the USSR as a "revisionist" power, Harkavy makes the point that the weapons market is highly relevant politically. This is especially the case in the postwar era when "outright arms aid has become an important instrument for the spreading and maintaining of diplomatic influence" (p. 147), while during the interwar period the arms trade was exclusively an economic enterprise. The author's assertion that the arms market has been gradually politicized is apparently correct. It is equally true, however, that the shift of both superpowers from granting weapons to selling them did not result in the depoliticization of the arms market.

The volume is not a flawless product, though its strong points clearly overwhelm its weaknesses. The book deals only marginally with the purely political consequences of the trade in weapons. This sin of omission is extremely common in the literature, and probably reflects the difficulty of evaluating–let alone accurately measuring–the political significance of the arms flow. Yet, when Harkavy distinguishes analytically between different weapons markets (monopoly, oligolopoly, and free market) he actually invites his readers to demand a more precise evaluation of the political results associated with these different structures.

Paradoxically, the strongest elements of the book–its theoretical awareness and relatively advanced status technically–also emphasize its major flaws: the need for tightening the theoretical structure (so that single hypotheses will form an integrative whole) and the need for more sophisticated research tools. In both respects, however, Harkavy's book is a better product than previous volumes dealing with the same subject.

ILAN PELEG

Lafayette College

Three and a Half Powers: The New Balance in Asia. By Harold C. Hinton. (Bloomington: Indiana University Press, 1975. Pp. 306. $15.00, cloth; $3.95, paper.)

The title of this book is misleading because fully 90 percent of its contents is a review of post-World War II international relations in Asia (excluding the Middle East) while only the concluding chapter deals with present and future power relationships and the prospects for peace and war in this vast area of the world.

Hinton is straightforward about the purpose of the book, however: the preface states that "my aim has been to write a book that would say something significant and interesting about Asia to the general reader, the student (whether in connection with a formal academic program or not), and if possible even the specialist in some aspect of contemporary Asian affairs" (p. vii). He is also deeply interested in trying "to understand and convey the reasons for American decisions and behavior" in Asia since World War II (p. viii).

For the most part, the author succeeds in his purpose; he has written a highly readable and reasonably unbiased account of what has happened in Asia since the Japanese surrender in 1945, and how the great powers have interacted there. Part one of the book, entitled "Asia in the Era of American Ascendancy" deals with the period up to 1969, when a new administration took office in the United States and the Soviet Union and the People's Republic of China engaged in a serious armed clash along the Ussuri River. Part two is labeled "Asia in the Era of Soviet-American Parity," and deals with events up to the middle of 1974. Here the author examines the Sino-Soviet agreements, shifting American priorities in Asia, Japan, India, and Southeast Asia in the early 1970s. The final chapter entitled "Conclusion: A Multipolar Balance?" might well have comprised an expanded final section of the book.

The study has a few obvious errors of fact and a number of interpretations of facts which will be challenged by Asia specialists. For example, on p. 268 the author states that the Association of Southeast Asian Nations (ASEAN) came into being in 1967 through the inclusion of Malaysia and Singapore in a regional grouping previously sponsored by Thailand, the Philippines and Indonesia. In fact, however, it was Indonesia that was brought into a grouping of Thailand, Malaysia and the Philippines, known as ASA, formed in 1961 and which Sukarno refused to join. Once Sukarno was ousted in 1966, the new Indonesian government made peace with its neighbors and, together with the newly-independent Singapore, joined ASEAN. Professor Hinton also refers to the "comparatively short-lived (October 1950–July 1953)" undeclared war between the United States and China in Korea (p. 53), a statement that will be challenged by the military who fought there, and his interpretation of how SEATO was founded in 1954 (p. 81) does not seem to be supported by the known facts. He states that Dulles stampeded Britain, France and the anti-communist states of Southeast Asia into the alliance by letting it be thought that he was "contemplating American military intervention to save Dienbienphu, although in reality he appears to have been considering nothing of the kind." My understanding is that Dulles could not persuade Eisenhower to intervene in Indochina in 1954 unless the British were willing to join the effort and that SEATO was born as a fall-back position when the

British decided to negotiate a settlement in Indochina instead of fighting for the French.

To Hinton's credit, he correctly predicted the collapse of South Vietnam after the American military withdrawal in 1973, but he is not as pessimistic about the effect of this event on the rest of Southeast Asia as are the adherents of the domino theory. He is optimistic about the future moderation of China's policies toward its neighbors and does not believe that Soviet influence will expand greatly as American influence declines in Asia. He does not see either China or Japan emerging as a superpower, but he believes both will follow constructive policies compatible with American interests.

Because so much questionable scholarship has been published on Asia in the past ten years, Hinton's effort to write a balanced account of what has happened there, and why, is a valuable contribution to knowledge and understanding of this quarter-century history of Asian affairs. Although it may not provide much that is new to the specialist, it should give a new generation of students a more balanced picture of what has happened in this key area of the world.

DONALD E. NUECHTERLEIN

Federal Executive Institute

The Future of International Fisheries Management. Edited by H. Gary Knight. (St. Paul, Minnesota: West Publishing Co., 1975. Pp. xiii + 253. Price not given.)

Agreement on a global management scheme for ocean fish is one of the key objectives of the current Law of the Sea Conference. To provide a comprehensive overview of the legal and economic issues bound up in the negotiations, H. Gary Knight, Professor of Law and Marine Sciences at Louisiana State University, has compiled these papers prepared by members of the American Society of International Law's Working Group on Living Marine Resources.

The historical context for conflicts involving international fisheries issues is skillfully described by Knight. He recounts the evolution of the current United Nations negotiations from the days of high seas' freedom and open access to all comers, through the 1958 and 1960 efforts to codify principles of international maritime law, and lays out the main conflicts that continue to frustrate agreement. The tough questions include global agreement on the objectives of fisheries management (e.g., food production, maximum sustainable yield of the fishery, net economic return, avoidance of international conflict) and the place of conservation and allocation in an international fisheries management regime.

Economics and politics, of course, are at the core of the fisheries issue. Where economic problems are given prominent consideration, however, political considerations are treated only by implication. Specifically, Jon L. Jacobson performs an impressive assessment of likely technological changes in the fish-catching business. He argues persuasively for flexibility in whatever kind of agreement the diplomats reach in order to adapt to technological changes that in many respects are responsible for making the traditional law of the sea obsolete. Jacobson's assessment must be read with Robert H. Newman's brief but provocative discussion of multinational investment in fisheries. New technologies and rising profits from sea food products is bound to be attractive to the big international corporations but the political and policy problems linked to these prospects are only dimly perceived.

Legal and economic considerations also have an important bearing on the kinds of decisions that must be reached. Douglas Johnston provides a detailed discussion of a generally neglected topic, namely, how a fisheries management treaty will be put into action if an accord is reached. Issues involve the relation to other international agreements, the impact of the Vienna Convention on Treaties on future international fishing arrangements, and specific problems of observance, application, and acceptance of the terms of the treaty.

The neglect of politics is most acutely felt in Lee Anderson's economic model of the optimal international fisheries arrangement. The basic economic principle governing such an arrangement is that "extra fish should be caught only when the cost of doing so is less than or at least equal to their market value" (p. 161). Like many economists confronting the fisheries management problem, Anderson postulates a world free of market imperfections and global consensus on maximum economic yield as an international management goal. Given these conditions, the criteria necessary for achieving international maximum economic yield require that: (1) countries continue to negotiate on allocation of fishing effort until neither can be made better off without making the other worse off; and (2) regardless of who owns the rights to fish, fishing effort should be produced in that country where production costs are the lowest. Political decisions are rarely so neat.

So many of these schemes—particularly the economic ones, but also the "Principles for a Global Fisheries Management Regime" enumerated at the end of the book—are offered as if the fishery question takes place in a political vacuum. "Fisheries management" is a misleading term. In fact, it is human behavior that is managed. And management implies authority, the power to make decisions and expect compliance. The forces bearing on these decisions—the record of past successes and failures; the likelihood of compliance with various management schemes; the nature of fishing interests within and between countries; and the relative places of political, diplomatic, and economic considerations—all must be analyzed and elaborated in order to provide the comprehensive overview of fisheries management issues promised here.

This neglect of politics is a troublesome characteristic of much ocean policy literature and is not unique to the Knight volume. Nonetheless, this is a most useful introduction to the problems of international fisheries management and is an excellent starting point for newcomers to ocean law and politics.

LAURISTON R. KING

National Science Foundation

German Politics under Soviet Occupation. By Henry Krisch. (New York: Columbia University Press, 1974. Pp. 312. $15.00.)

During the months immediately following World War II the ultimate postwar aims of the Soviet Union were not clearly defined. In contrast, the objectives of the Communist parties of eastern Europe, under the leadership of those who returned with the Red Army, were much more precise. Their aim was to attain firm control over the levers of power and to consolidate that power.

In all countries under Soviet occupation the attainment of the unification of the Social Democratic and Communist parties was for the latter a *conditio sine qua non*, for without it they would have never achieved their dual objective: the ultimate domination and organization of the working classes along Communist lines, and the relegation of the Social Democrats to a subordinate role in the envisaged unified parties. In order to facilitate the organizational unity of the two working class parties, ideological streamlining had to be postponed to a later date, as it happened in most of the East European countries. In contrast to this delaying strategy, the East German Communists started the drive for party unification almost as soon as the war was over.

It is this background against which Professor Krisch's study should be evaluated. As he states, the study is a "detailed examination of Soviet policy in the immediate postwar period as that policy affected German political life" (p. 2). He further states that "the subject matter and context of that policy were firmly set in a framework of historical antecedents, national peculiarities, ideological speculation, and personal reactions" (p. 3). These are the broader propositions of the study. In specifics, part of the study "focuses on the process by which the Communist and Social Democratic Parties were first separately established and then merged in the Soviet Zone between April 1945 and May 1946" (p. 2).

Building on these premises, Dr. Krisch presents us with a commendable case study of the unification of the Communist and Social Democratic parties in the Soviet occupied zone of Germany. In this endeavor he takes a dual approach: that of a historian and that of a political scientist. As a historian his narrative begins with the first, abortive, attempts by the two ideologically incompatible parties at reaching an understanding, following the establishment of the Third Reich. His flawless historical presentation further informs the reader of the new beginning of political life immediately following the termination of hostilities, along with the differences, intrigues, and ideological dilemmas that both parties faced in their endeavor to achieve their respective objectives which, in most cases, did not coincide.

As a political scientist Krisch leans heavily on rare source material from which he quotes so freely that he almost overwhelms the reader. He clearly identifies the important issue areas, each of which was the source of potential conflict that under the given political situation had to end later in an almost unilateral compromise. Among other issues, he discusses the Soviet encouragement of early party formulation, followed by restrictive methods and harassment, the diametrically opposite stand on the national issue of the Oder-Neisse line, and the Communists' drive for the achievement of early institutional uniformity, which implied the future imposition of ideological conformity on the SPD. Krisch analyzes in a relatively sophisticated manner the dilemma of the establishment of an all-German SPD and the political consequences of its unattainability.

Yet, it is this aspect of Krisch's discussion with which this reviewer takes some issue. In discussing the differences between the two leaderships, the author implicitly blames Schumacher for the failure to establish a common platform between the Western and Eastern SPD. Suffice it to say that under the prevailing conditions in Germany, in light of the ideological differences between Schumacher and Grotewohl, and assuming that such a step would have been contrary to Soviet aims, an all-German SPD in 1945–46 was as unthinkable as it was at any later date.

Nor can one fully agree with some of the conclusions of this otherwise correct analysis. It is difficult to accept that the Soviet policies in the zone helped to maintain "its commitment to the creation of a viable four-power control machinery in Germany" (p. 200), or the creation of "an instrument of potential leadership throughout Germany" (Ibid.). In early 1946 neither the world nor Germany were ready for unification, much less for the acceptance of a Communist-dominated political leadership. Secondly, one has to keep it in mind that it was the Soviet Union and not the Western occupying powers that first undermined and later sabotaged the four-power machinery supposedly responsible for the governance of Germany. Finally, the whole issue should be seen against the background of the just-emerging Cold War which was virtually unleashed by Stalin's speech in the summer of 1946.

Aside from this little cavil, however, it must be said that Krisch's work will serve as a valuable source material for historians and political scientists as well interested in the strategy and practice of Communist takeover. Moreover, strange as it may seem, the book also has an important message for the present day when the Communists, in the Latin belt of Europe, albeit no longer on Moscow's leash, are openly bidding for power.

LASZLO GORGEY

Benedict College

Germany and Poland: From War to Peaceful Relations. By W. W. Kulski. (Syracuse, N.Y.: Syracuse University Press, 1976. Pp. xxii + 336. $22.50, cloth. $9.95, paper.)

The victim has a longer memory than the oppressor. In the case of German-Polish relations, the memories of both sides are burdened with a long and complicated historical past; and the most recent diplomatic developments aimed at conciliation and mutual understanding come at the end of an extensive period of conflict, tentative probing actions, and guarded approaches toward a mutually acceptable modus vivendi. The Cold War, and the division of Europe resulting from it, raised particularly sensitive issues between Poland and West Germany and placed these issues in a regional political context that was of central importance to the two superpowers, the United States and the Soviet Union, and their respective alliance systems.

One of the many merits of W. W. Kulski's book on German-Polish relations is the emphasis it places on historical developments and their psychological and political impact on the more recent past and the present. After an examination of the convoluted historical movements in the medieval period and the era of Poland's partitions, Kulski turns (in Chapter 1) to an overview of the nineteenth century, World War I, and the Weimar period. Chapter 2 deals with World War II; and the subsequent chapters (3 to 12) trace the origins of the Oder-Neisse issue and present a chronological account of German-Polish relations during the Adenauer era, of the tentative revisions of Germany's eastern policy during the Erhard administration and the Grand Coalition government, and of the culmination of these policies in Willy Brandt's *Ostpolitik.* At appropriate junctures, Kulski also deals with domestic responses to these developments, in Poland as well as in West Germany, and his treatment of the Bundestag debates over ratification of the Warsaw Treaty is an example of careful and responsible political analysis.

Kulski, who has relied primarily on the available documentary materials and secondary sources, gives a thorough, orderly, and by and large judicious account of the tortuous history of German-Polish relations in the postwar period. There are no startling revelations, implausible speculations, unsupported conclusions or ill-founded predictions. In many respects, this style of analysis is well-suited to a subject that has evoked highly emotional responses on both sides and continues to be a touchy political issue. Nonetheless, the reader is left somewhat dissatisfied. Important as psychological dimensions have been in Polish-German relations, a great deal of hard-headed realism has characterized the dealings between Warsaw and Bonn during the last decade; and the issue of German and Polish historiography and textbook revisions (much stressed by Kulski in his concluding remarks), although symptomatic of long-standing suspicions and misperceptions on both sides, most likely should be viewed in the context of more practical considerations that have motivated both Germany and Poland. Such considerations are not always given full weight by Kulski. For example, after stating that "the main Polish interest in normal relations with the [Federal Republic of Germany] is economic" (p. 283), Kulski devotes only several perfunctory pages to economic issues. Military-strategic questions get similar short shrift, even when they are primarily of political significance, as exemplified in the various disengagement plans of the late fifties with which Polish Foreign Minister Adam Rapacki became prominently identified. Throughout the book, one misses a discussion of (and sensitivity to) larger political contexts that provided the background for Polish as well as German diplomatic activities, especially in the area of alliance politics which, at times, rather narrowly circumscribed Warsaw's and Bonn's maneuverability in dealing with one another. There is hardly any mention of the European Security Conference, not to speak of the element of timing that might have prompted the West Germans to speed up their negotiations with the Soviet Union and Eastern Europe in order to complete treaty arrangements in a more or less bilateral context rather than under the pressures created by a multilateral forum. Nor is there a discussion of American policy toward Eastern Europe and the apparently somewhat differing views that the United States and Western European countries hold about a desirable relationship between the Soviet Union and its East European neighbors: it is, after all, still a central paradox in U.S.-Western European relations that the United States, in dealing with the Soviet Union, is compelled to accept the division of Europe, while most Europeans are anxious to see it overcome. Another theme that might have been stressed is the striking complementarity that existed between Polish national interests vis-à-vis West Germany (especially on the Oder-Neisse issue) and the Soviet interest in bloc cohesion, ideological purity, and a Russian role as protector of Eastern Europe—*Realpolitik* and ideology became mutually supportive and created a powerful incentive for bloc cohesion under Soviet hegemony.

Even though Kulski's analysis of Polish-German relations might have been enriched by a more thorough consideration of the larger contexts in which Bonn and Warsaw had to operate, the book is a welcome addition to the historiography of Europe in the decades since World War II. Kulski writes clearly, he is well-informed, and his subject is well served by the lucid and responsible treatment he brings to bear on it.

WOLFRAM F. HANRIEDER

University of California, Santa Barbara

The "Tar Baby" Option: American Policy Toward Southern Rhodesia. By Anthony Lake. (New York: Columbia University Press, 1976. Pp. xiii + 316. $17.50, cloth. $5.95, paper.)

Anyone interested in southern Africa or in how our foreign policy is made will appreciate this excellent analysis of American policy toward Southern Rhodesia. The author has made good use of his contacts with former colleagues inside government by conducting wide ranging interviews which turned up much new and interesting information.

The inevitable dilemmas and compromises in foreign policy making are succinctly highlighted in the second chapter on the international responses to Ian Smith's declaration of Rhodesian independence in 1965. Lake elaborates the various kinds of dilemmas faced by the British, the African states (notably Zambia) and by numerous other powers, and he shows how the resulting compromises weakened the United Nations by asking it to do a job and then not allowing it to do it.

The next chapter is a perceptive account of bureaucratic in-fighting over our Rhodesia policy between 1965 and 1968. Cabinet-level officials regarded the Rhodesian problem as "just a little mess the British had on their hands" (p. 61). At the working level, it was taken more seriously although opinion was divided. Officers supporting majority rule were found in the State Department's Bureau of African Affairs (AF), the Bureau of International Organization Affairs (IO), the Office of the Legal Adviser (L), the U.S. Mission to the United Nations (USUN), the Bureau of Intelligence and Research (INR), and in 1965 in the National Security Council staff in the White House. A more conservative group, focusing on our economic and strategic interests in southern Africa, included NASA and the Departments of Commerce, Defense, and the Treasury, and sometimes the Bureau of Economic Affairs in the State Department (E). A third group were the "senior officials who wanted a low-risk, low-cost policy that would let them get on with more important business" (p. 69). Under-Secretary of State George Ball was its dominant figure and it included State's Bureau of European Affairs (EUR), although EUR, with some support from INR, went further than Ball in supporting British policy against the Smith regime. Lake puts some flesh on the bones of this bureaucratic structure with occasional vignettes of the personalities involved, a risky but interesting effort. His account of the role of Deputy Assistant Secretary of State J. Wayne

Fredericks is misleading because it omits so many aspects of Fredericks' activities and motives. Fredericks lost battles, to be sure, but not because he was "romantic" (p. 65).

Meanwhile, right and left-wing pressure groups were lobbying extensively in Congressional and administrative circles but, in Lake's view, they aroused little concern among American officials whose policy of compromise reinforced a British policy "remarkably successful in the terms of the senior officials who made it" (p. 120). It bolstered the British policy of sanctions against Southern Rhodesia through the United Nations and avoided excessive recrimination from Africans.

Early in 1970 President Nixon accepted Kissinger's recommendations for more communication with white minority regimes, for a partial relaxation of American measures against them, and for a cooler rhetoric by U.S. delegates to the United Nations. This presidential decision was the Tar Baby Option which rose out of National Security Study Memorandum 39. It got its name from its opponents in the State Department who argued that it was a sticky option which would be difficult to abandon if it did not work. The Tar Baby "tilt" toward the white regimes, based on the fallacious premise that "the whites are here to stay," was soon outmoded by the independence of Angola and Mozambique.

The "Tar Baby Option" makes a catchy cover but one sometimes feels that the author is sandtrapped by his title when he includes material that has little or nothing to do with Tar Baby. Lake acknowledges at one point (p. 133) that not all the steps he describes are directly related to it. The book excels, however, as a study of American policy toward Southern Rhodesia, including several meticulous chapters probing the motives and results of the Byrd Amendment and the efforts to repeal it.

VERNON MCKAY

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Japan Faces China: Political and Economic Relations in the Postwar Era. By Chae-Jin Lee. (Baltimore: The Johns Hopkins University Press, 1976. Pp. xiv + 242. $12.50.)

The impact of America on Japan's relations with China from World War II into the 1970s has been profound; today, an understanding of United States relations with either Japan or China requires a grasp of recent China-Japan relations. Professor Chae-Jin Lee offers in this tightly written book the first thorough study of post-1945 Sino-Japanese political and economic relations; it will long stand as a basic source for specialists in East Asian politics, American foreign policy, and international relations.

The author's comprehensive analytical framework renders less labyrinthine the "tortuous evolution" of official and unofficial contacts during six periods: "(1) formal estrangement, 1949–52; (2) gradual relaxation, 1953–57; (3) total collapse, 1958–62; (4) limited adjustment, 1963–65; (5) extreme hostility, 1966–70; (6) comprehensive reconciliation, 1971–72" (p. 186). The latter period commenced with the Japanese recovery from the "Nixon Shock" visit to the People's Republic of China (PRC), resulted in the 1972 establishment of full diplomatic relations, and set the tone for Sino-Japanese relations into the late 1970s. These vicissitudes are assessed in terms of the interplay of the "foreign environment" (the Cold War; the two governments' perceptions of each other; respective interactions with the United States, the USSR, the two Koreas, and Taiwan), the general "domestic environment" in Japan (public opinion; activities which focused on China and Taiwan on the part of government institutions and interest groups), and "intraparty conditions" (the ideologies, factions, organizational channels and decision-making characteristics of Japan's political parties) (pp. 3–22).

This work analyzes in detail the roles of Japan's five political parties: the perennially ruling Liberal-Democratic Party (LDP), which holds a narrow majority of the 511 seats in the Diet's powerful House of Representatives following the December 1976 elections; the opposition Japan Socialist Party (JSP, 123 seats); and the three minor opposition parties, the Japan Communist Party (JCP), the Clean Government Party (CGP or Komeito), and the Democratic-Socialist Party (DSP). (The New Liberal Club emerged after the period covered by this book.) The LDP's (and thus the government's) China policy was influenced most by "the shifting U.S. position in Asia," less by intraparty factional politics, and least by extra-party domestic factors. On the other hand, the JSP's policy owed less to the "foreign environment," very little to the "domestic environment," and most to the party's internal politics (p. 189). Positions on the China question cut across party lines. For example, both anti-mainstream LDP leaders (e.g., Matsumura Kenzo) and opposition politicians (e.g., the JSP's Narita Tomomi and the CGP's Takeiri Yoshikatsu) made informal but diplomatically notable visits to China; some conservatives fostered normalization, while the JCP, after Miyamoto's 1966 talks with Mao, exchanged verbal blows with the Chinese leadership.

Amicable relations rest on a fragile domestic political base in both nations, the author tells us, in light of their "bitter historical legacy, ambivalent attitudes, and irreconcilable systems" (p. 192). Moreover, changing U.S. and Soviet policies will continue to affect the strategy of the Sino-Japanese situation. Japan has been the PRC's principal trade partner since 1965; but economic cooperation sufficient to counterbalance the impact of the two superpowers on their relationship "would require China's conceptual breakthrough in accepting Japanese capital and technology for massive collaborative projects" (p. 193). History justifies the PRC fear of dependence on the industrialized powers; but the PRC seemed to be moving very cautiously toward some capital and technology import liberalization in 1977. Other problems are Japan's de facto relations with Taiwan, conflicting claims regarding the Senkaku (Tiaoyü) Islands and the oil of the continental shelf in the East China Sea, and potential competition over interests in Korea and Southeast Asia.

The author has made judicious use of Chinese sources (as available), as well as Japanese and western sources in picking his way carefully through the minefield of biases and misinformation—found in all sources—which have affected modern diplomacy and American public discourse on East Asia. However, I have two criticisms: this book's perfunctory (one page) handling of pre-1945 history deprives the nonspecialist of sufficient perspective on subsequent patterns and on the occasional cryptic comments on the prewar period; exaggerations creep in concerning the closeness of past and present Sino-Japanese cultural linkages (e.g., pp. 1, 6, 9, 72, 194). The polite diplomatic contentions of both official and private spokespersons in recent years are not verified by history or attitude patterns emerging from post-1945 opinion poll data in Japan. Japan's premodern relations with China were spasmodic, but Japan incurred a cultural debt (such as exists between some European nations); from 1850 to 1950, the flow was principally from Japan to China, with some good and much ill effect, as Japan joined the industrialized imperialist nations and mixed western culture with a Japanese base. Relatively few Chinese and Japanese are knowledgeable about each other's systems or languages (English is the second language of both). As elsewhere in the world, geographical proximity has implied no stable tendency toward easy friendship in East Asia.

For those reflective about either the slowness or the suddenness of America's development of cordial relations with the PRC, about the low level of public education on East Asia in spite of three wars there in 35 years, or about the strength of lingering misperceptions about Japan among American intellectuals (suggested by a recent poll), this volume can be an aid in bringing one's imagery regarding East Asian politics more into line with reality.

The Appendices contain critical documents in Sino-Japanese relations, from Prime Minister Yoshida Shigeru's famous letter to Ambassador John Foster Dulles (December 24, 1951) expressing Japan's intent "to have a full measure of political peace and commercial intercourse with China," to the Joint Statement of the two governments (September 29, 1972) finally normalizing relations. Numerous tables supply data on related public opinion in recent years, organizations, trade and political party activities; very few typographical errors mar the text.

In sum, this study represents an excellent combination of area expertise and political science; it is an indispensable guide to one of the world's critically important bilateral relationships.

LAWRENCE WARD BEER

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Nearing the Crossroads: Contending Approaches to Contemporary American Foreign Policy. By Reynold Levy. (New York: Free Press, 1975. Pp. x + 180. $9.95.)

Authors and publishers often search for new frameworks or new approaches to the study of phenomena in the hope that some magical key will be found to unlock a door leading to immense treasures. Once found, of course, the treasures are those to be generated by sales of the imaginative new book. But, as in most areas, magic is sleight of hand and miracles occur only for the true believer. New frameworks and new approaches which merely reorganize and rearrange observations are neither magical nor miraculous. And the coffers of the author and publisher will not be transformed necessarily because of the publication of the book.

Reynold Levy's book *Nearing the Crossroads* is such an attempt to impose a new framework and thence discover new realities about American foreign policy. The new framework, actually, is not all that new; neither is it much of a framework. Levy's device is to bifurcate approaches to the study of American foreign policy as domestic or systemic. Domestic approaches concentrate on the American polity, environment and experience. Systemic approaches concentrate on the international system, relations among nations and between international governmental organizations, international non-governmental organizations, and other state and non-state actors.

Having adopted the framework, all students of and approaches to American foreign policy are classified either as systemic or domestic. The author concentrates throughout on developing the distinction between the two contending approaches and locating various authors in that framework. Having asserted that all authors or students of foreign policy are either domesticists or systemists (Levy's terms), Levy leaves no room for combinations of the two or variations in approaches.

Developing a framework and adhering rigidly to it is admirable if the framework fits and adherence does not do violence to our understanding of the field. However, Levy adheres to his framework despite its inadequacy to accommodate diverse approaches to American foreign policy. In making things fit into that framework, he is led to some rather strange conclusions, e.g., that domesticists are isolationists and systemists are internationalists. Thus Fulbright, Kennan and critics of the Vietnam war are isolationists, while Claude, Hoffmann, and defenders of the war policy are internationalists. This is a logical outcome of a rather inept application of the framework. It results from his basic premise that those who focus upon domestic conditions in the United States are principally interested in domestic concerns and wish to optimize American interests with the least possible external (systemic) interference or diversion.

Unfortunately, this book's value rests upon the quality of Levy's framework. If scholars or public officials really fit neatly into the two categories, this study might have warranted recommendation to students and scholars. But one would be hard pressed to classify Senators Mansfield or Fulbright as isolationists except through specious oversimplification. Substantive discussion of foreign policy is not to be found in Levy's book, which leaves it virtually devoid of redeeming social value.

EDWIN H. FEDDER

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Coping with the Oil Crisis: French and German Experiences. By Horst Mendershausen. (Baltimore and London: The Johns Hopkins University Press—Resources for the Future Inc., 1976. Pp. xvi + 110. $7.50, cloth; $2.95, paper.)

With time, mankind usually comes to understand what befalls humanity. This timely study brings to light the French and German politico-economic processes and substance that transcended the 1973 oil crisis. It picks up where the excellent Romano Prodi and Alberto Clô article on Europe in the crisis trails off ("The Oil Crisis: In Perspective," *Daedalus*, Fall 1975) and goes on to explain what changes have occurred in French and German oil politics, taking these changes in context.

Starting from the breakdown in the pre-1973 structure of oil relations that emerged following World War II, with the victor governments in the role of global oil market "police" and the oil majors as market "regulators," Mendershausen traces the differing French and German approaches to their national energy economies. Woven into this is the manner in which foreign oil firms are viewed in each country—the French hostile and wary, the Germans more hospitable but careful—and the specific governmental policies dealing with domestic and foreign oil firms. The French *marché ordonné* differs substantively from the German *Marktordnung*, though their English translations may seem identical.

Mendershausen's analysis of the basic French and German situations is succinct. The French were striving to: have French-owned oil firms' global crude production meet their domestic consumption (this occurred only during the 1960s French control over Algerian sources); limit foreign oil firms' domestic market shares to less than 50 percent; have total French refinery sales exceed domestic demand. Coupled to this, the French had decided to allow coal production to be replaced by oil as quickly as French oil firms' strikes came on stream, a fact not made explicit by Mendershausen. The Germans, who did not have access to any colonial (or former colonial) oil base, sought as their primary oil policy "... to dampen the market forces advancing the substitution of oil for coal" (p. 24). Like the French, their coal policy (protectionist) was linked to their oil policy.

This analysis is then placed into the context of the global oil security search which considered the interrelationships among the oil firms, among consumer governments, between consumer governments and the oil firms, between producers and consumer governments, and among the oil cartel members. From Mendershausen's *considerations*, a picture emerges: consumers searching steadfastly for cheap-but-stable supplies (a paradoxic relationship); oil firms searching for less hostile conditions; producers searching for stable wealth. His comment, "the actors find themselves in a stormy state of no peace-no war in which they try to position themselves for either eventuality" (p. 66), is an accurate summation of the French position. M. Froment-Meurice, head of the economic side of the French Foreign Ministry, told this reviewer that in early 1974 *"les americains avaient choisis une politique pour faire la guerre"* and that his primary concern had been to be prepared. Although Menderhausen's symbolic references throughout to "peace," "war," and the OPEC "ambush" are meant to link his analysis of the oil political economy to some implicit winning or losing or to certain elite perceptions in this struggle, this detracts from his efforts to further understanding of complex processes underway in a contemporary period of great uncertainty in the minds of all the actors.

Throughout, multivariate factors and their dynamics are considered–a strength of this volume. After consideration of consumer options–self-sufficiency (in the vein of *sauve qui peut*) or collective bargaining–French and German response to the 1973–74 winter events is examined. Gaullist France took the OPEC challengers' side to attempt self-sufficiency. Bonn's response was relatively neutral and based in collective consumer bargaining, more sympathetic toward U.S. foreign policy. The French, designated friends of the Arabs, were "exempt from supply restrictions" (p. 68) whereas the Germans received restrictions, not the total embargo like Holland. With such assurances, the French were casual about immediate needs; the Germans, though, saw the need for reduced consumption. The French shot ahead with all sorts of bilateral negotiations, seeking any means to bypass ARAMCO in Saudi Arabia through government-to-government deals. The seeking of equality with ARAMCO was disliked by the Saudis and France got no special long-term contract. Giscard d'Estaing dropped this direct approach of Jobert and sought more loosely linked economic transactions in Iran in order to manage the burden of quadrupled oil prices. The West German response continued in the directions it had been going beforehand–indirect economic projects, especially establishment of basic industrial infrastructure plants, in order to get energy supplies (oil and gas). The Germans were also successful in Saudi Arabia, getting an allotment of Saudi participation oil. Both France and Germany were able to alleviate part of their problem through such loosely linked economic projects. In this, the German Deminex pulled itself up to an equivalent footing with CFdP in the Mideast.

The response was also directed at foreign oil firms. The French considered reducing foreign firms' quotas in their *marché ordonné* but realized they might cut off their noses to spite their faces by losing U.S. majors' supplies too quickly. With considerable restraint, Bonn avoided interfering in their market and sought oil firm cooperation in exploration ventures. Yet the Germans were in fact seeking insurance against possible future failure of the oil majors to supply their needs. Bonn increased governmental support of Deminex and purchased controlling interest in Gelsenberg (or GBAG) which was then merged (for a number of reasons) with VEBA to create a "national" oil firm on the lines of ELF-ERAP. Thus Bonn approached Paris' response style without adopting it.

At the international level, French and German styles were reflected consistently. They opposed each other at the Washington Energy Conference, in the formation of the IEA, in the changing of OECD energy programs, and in the quest for a common European Communities (EC) energy policy. These policy differences did not preclude transnational cooperation among EC members, but they did prevent any joint response to the oil crisis.

That Horst Mendershausen undertook to get behind the French and German façades in the oil crisis is commendable; his results are noteworthy and of great value. This reviewer's own EC energy policy research went through many of the same personalities and institutions that Mendershausen did, some at the same time. In a preparatory paper, "October in Western Europe" (Rand, 1974), he laid out in great detail the various strategic elites canvassed. Moreover, he found "everywhere a sense of shaken foundations of governments, financial systems, intergovernmental and industrial relations." Through his eyes, European elites had perceived Western political frameworks inadequate to the oil challenge. The explicit psychological pessimism he saw here is implicitly woven into the volume reviewed. Also, why he thought Andrew Shonfield's *Modern Capitalism* was a suitable analytic starting point becomes clear. Mendershausen clearly was qualified to undertake this effort. If there is any weakness, it lies only in the lack of clear policy prescriptions for industrial nations in his "Summary and Conclusions" (Ch. 5). Some good thought is needed here, as industrial nations are at a crucial point—whether to go ahead with massive traditional energy production and distribution schemes (especially nuclear) or to move fully toward end-use schemes, with their greater conservation (and environmental) benefits.

What Mendershausen has brought us is a skillful comparative case analysis of the French and German political economies in response to the 1973 oil crisis. Additionally, it has been placed properly in its systemic context. The result is central to scholars studying advanced industrial societies generally as well as to those examining France and the Federal Republic of Germany in particular. Furthermore, it gives oblique insights into the conflict resolution of two industrial states entering the postindustrial era. If such a crisis had been encountered in bygone days, it would have surely forced a Franco-German alliance to curtail any oil nation cartel. No colonizer has ever permitted ingratious colonies to usurp relationships; only coercion has achieved this.

For those proposing policies today to cope with the problems of international energy cooperation, whether at the domestic or global level, this book is "must" reading. Its cogent, coherent format cuts through the haze raised by numerous recent advocacy studies. It gives policy makers a clearer picture of two industrial nations' policy responses in a time of perceived crisis—an experience that can help others, superpowers included, avoid major energy policy pitfalls on the global scene.

ROBERT A. BLACK, JR.

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The International Politics of Natural Resources. By Zuhayr Mikdashi. (Ithaca and London: Cornell University Press, 1976. Pp. 214. $12.50.)

There are two central trends in the world of natural resources of the late 1970s. One is the virtually universal ability of countries where the resources are located—be they highly industrialized (the United States for soybeans) or rapidly emerging (Saudi Arabia for oil) or least developed (Papua-New Guinea for copper)—to dictate the economic and political terms on which those resources will be made available to others. The second is the readiness of most multinational enterprises, wherever their head offices may be located, to collaborate actively with such host-country actions.

In *The International Politics of Natural Resources,* Mikdashi provides a wealth of material which will help both experts and students to assess these trends. Indeed, he comes close to recognizing the trends themselves. But his sympathies for the developing countries, laudable though they are, seem to have deterred him (as they have many other writers) from pushing the facts to their logical conclusions, and thus prevented his writing the definitive book on the subject, as might have been expected.

Mikdashi is at his best in describing OPEC, on which he has previously written in great detail. His compendium of the different types of markets which exist (or do not exist) for a number of key primary products, and of the efforts of producing countries to seize greater control over the marketing of their commodities, is extremely useful. His exposure of the inconsistencies of the United States, in railing against the embargo of OPEC but blithely applying its own to farm products, is telling. And many of his policy proposals for smoothing the international flow of natural resources are both sound and creative.

The book is disappointing, however, because it fails to provide much analysis of its own data. Mikdashi could readily have concluded, for example, that the strongest associations of producing countries have emerged in industries which were already highly concentrated in the private sector (oil and bauxite compared with iron ore and tin, where there may in fact be more oligopsony than oligopoly). His data reveal that strong cartels reject producer-consumer collaboration because they do not need it (again, oil and bauxite), while weaker groups (e.g., copper and tin) seek help from consumers because they do. But Mikdashi fails to pursue the comparative analyses which his data help to make possible.

Surprisingly, Mikdashi also fails to come to grips with the debate over whether "more OPECs" are likely despite his earlier contribution to that debate ("Collusion Could Work," *Foreign Policy,* Spring 1974). He rightly shows that OPEC does not derive its strength from any political (i.e., anti-Israel) alliance and has succeeded despite massive differences among its members on a wide range of issues, but he fails to note that this bodes well for other producer efforts and even denies that OPEC itself is a cartel (p. 77). He neatly lays out the variables which influence the ability to cartelize (p. 197), but fails to apply them to his earlier description of the several producers' efforts. And his appraisals of IBA and CIPEC—the groupings of bauxite and copper producers—are too negative (see, respectively, Bergsten in the July/August

1976 *Challenge* and Mingst in the Spring 1976 *International Organization*).

Part of the problem, inevitably, is that no book can hope to keep up with events in an issue area as fast-moving as this one. For example, Mikdashi is unable to take account of the sharp shift in U.S. policy toward international commodity agreements, dating from May 1975. But some of his errors are more pervasive. The basic disputes on raw materials issues are between governments of exporting and importing countries, *not* between "businesses and governments" (p. 138). The growing symbiosis is between transnationals and *host* governments, not between the firms and "their parent governments" (p. 41). Mikdashi appears to recognize (p. 204) that widespread emulation of OPEC would have a *regressive* effect on international income distribution and hurt the poorest people in the poorer countries, as does OPEC itself, but he still seems to support it. Minor mistakes include a failure to recognize that all home countries of multinationals defer taxing their earnings until repatriated (p. 151), and a statement that the United States denied most-favored-nation treatment to OPEC countries in retaliation for the oil embargo (p. 194).

The fundamental shortcoming of *The International Politics of Natural Resources*, however, is its apparent tendency to let political sympathies deflect scholarly judgments. Mikdashi recognizes the "dramatic improvement in LDCs' bargaining power" (p. 78). He understands that developed host countries have effectively controlled incoming investors (p. 148). He acknowledges the well-known Adelman view that the oil companies have become agents of OPEC (p. 169). But he still concludes that the firms are the dominant actors. The reason may be simply that any alternative conclusion would undermine the widespread sympathy now enjoyed by the developing countries on these issues and belie the increasingly anachronistic image of the multinationals as autonomous and omnipotent forces in world affairs.

Mikdashi's explicit policy thrust is "improving the frameworks for the distribution of the gain from primary trade in favor of LDCs. . ." (p. 187) and "growth in [LDCs'] real export earnings" (p. 188), and he wants to "redress the balance of international economic relations which now favor the industrial countries" (p. 196). Such an approach would obviously be even harder to sell if it were widely perceived that such redistribution, growth, and redressing were already occurring, on a much more healthy basis, through the individual and collective actions of the LDCs (and other producing countries) themselves—frequently aided, not hindered, by the multinationals. Therein lies the major tension within *The International Politics of Natural Resources*, as well as within the international policy debate on these issues today.

C. FRED BERGSTEN

The Brookings Institution

Peace Soldiers: The Sociology of a United Nations Military Force. By Charles C. Moskos, Jr. (Chicago: University of Chicago Press, 1976. Pp. 171. $11.00.)

Pioneered, I suppose, by Morris Janowitz, military sociology has caught on and grown modestly in the past 20 years so that dozens of sociologists and lesser numbers of psychologists and political scientists now specialize in trying to understand what makes soldiers tick. How and why did they become soldiers? What kinds of people are they? What are their beliefs and attitudes? Their values? What do they think of their work, their societies, themselves? How and how well do their organizations work? What kinds of internal cleavages exist in them? How do these armed forces fit into their respective societies?

Writers grappling with these questions focus primarily on the soldiers and armies of their own countries, though there have been efforts to compare the warriors of several nations. Now, though, a different, new and interesting subject for such sociological study presents itself: the international peacekeepers, soldiers contributed by their governments to one or several of the dozen or so peacekeeping operations mounted since the late 1940s under United Nations auspices. These UN peacekeeping operations—as political and quasi-military operations—have been extensively studied and written about in case study and comparative volumes.

Moskos, however, has given us the first detailed and systematic look at the peacekeeping *soldiers* themselves. Having spent a year in field research, observing the peacekeeping operation, conducting formal interviews with officers, and informally socializing with enlisted men (but not, alas, formally surveying their attitudes), Moskos is able to tell us a good deal about the soldiers of UNFICYP—the complex United Nations peacekeeping operation established in 1964 on the Mediterranean island of Cyprus which is still there, making its valuable contributions to promoting peaceful and normalized relations between Greek and Turkish Cypriots.

How well did these soldiers—coming from half a dozen middle powers and trained as soldiers to use violence against the enemy—adapt to UNFICYP's peculiar constabulary role of using minimal force in a situation demanding aloof impartiality toward the disputants, none of whom is supposed to be seen as an enemy? Quite well, in fact, according to Moskos (and the disputants, too), mostly because UNFICYP's soldiers increasingly come to appreciate the peacekeeping value of their constabulary tactics in field situations. And while they might chafe at political limitations and "bitch" about pay and conditions, most have also apparently been able to see soldierly honor and a high level of professionalism as realizable even in the constabulary situation, perhaps because today's national soldiers are more of the managerial than heroic stripe.

Did UN service make the soldiers any more internationalist in their political views? Quite the contrary, Moskos tells us. When asked whether serving with UNFICYP had "changed any of your ideas about the need for a stronger United Nations or perhaps even a form of world government," most said they either had not changed or had, if anything, become less internationalistic.

Perhaps paradoxically, the UNFICYP soldier's skepticism about international and supranational approaches to regional or global order grew hand-in-hand with his ability to adopt and appreciate constabulary tactics in those special circumstances calling for them. To some this paradox will be disturbing; but to this reviewer it seems encouraging evidence of a capacity for thoughtfulness, flexibility, and wisdom among the world's warriors.

JAMES A. STEGENGA

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National Politics and International Technology: Nuclear Reactor Development in Western Europe. By Henry R. Nau. (Baltimore, Md.: The Johns Hopkins University Press, 1974. Pp. xvi + 287. $12.50.)

Several distinguished studies have demonstrated that, contrary to many scholarly and diplomatic expectations, the development of peaceful uses of the atom has not been ripe for international collaboration. Instead of relatively technical, cooperative, apolitical decision-making, nuclear power programs have been marked by strongly competitive, politicized, and nationalistic behavior. Thus, the resilience of the nation-state and the persistent importance of national self-interest in important economic and technological matters have been underscored.

Henry Nau's book on the growth of nuclear power programs in Western Europe is the most recent and complete chapter of this lineage of theoretical argument and empirical research on nationalism's persistence. It contributes to the empirical record, and it lucidly extends the argument about the respective roles of political and technological factors in international relations.

Five case studies of nuclear reactor development in Western Europe constitute the empirical record. Each program involved a different technology and a different organizing strategy for reactor development. Each one failed or disappointed greatly. None achieved the creation of an integrated, self-sufficient European nuclear power industry, nor did any of the five notably strengthen Atlantic or West European political solidarity. The accounts of the decline of EURATOM, the slender progress of DRAGON, the muddle of the heavy water reactor projects, the bitter fights over European versus American reactor designs for current power plants, and the uneasy diplomacy over the fast breeder reactors are indispensable to scholarly specialists. They are tightly introduced by chapters on previous technology development disputes in the Western alliance, and on the idiosyncratic political and economic determinants of national nuclear programs. These accounts establish a new standard for the study of atomic collaboration, although further research may produce corrections (e.g., revision of the relationship ascribed to utilities and their suppliers) and additions (e.g., emphasis on the effects of competition between natural gas and electric utilities).

However, the theorizing based on the studies is likely to prove more controversial. Nau concludes that, first, there is no technological imperative dictating either international cooperation or apolitical decision making; and, second, political factors are the most crucial determinants of international cooperation. As a simple refutation of functionalist or crude neo-functionalist hypotheses, the argument is persuasive. As a more general explanation of the political determinants of international collaboration, the argument poses difficulties.

Nau believes nuclear power development illustrates the perennial conflict between "dominant" and "weak" countries. Dominant states press their technological advantages and more general foreign policy interests by promoting ambitious schemes for international cooperation. They offer sidepayments to weak countries in return for loyalty to the dominant's technology and goals. Weak states resist perpetual dependence, accept limited international pacts geared to specific functional goals, and flexibly choose partners to gain the most help at the lowest cost. In nuclear technology the United States has always been a dominant power. Britain and France have sometimes been dominant, while West Germany, once weak, is now strong but reluctant to pursue dominance. Regardless of specific role occupants, role conflict implies failure to cooperate for all but nascent or declining technologies. The few vital exceptions are usually administratively shackled by political bickering. These dynamics largely explain the failure of nuclear power projects in West Europe.

Nau's thesis suffers from overly broad categorizations of politics and technology and from difficulties posed by equating the "dominance" of America, France and Britain for the purpose of theorizing. More crucially, while Nau self-consciously focuses on high-level politics, he never fully confronts a competing hypothesis suggested by economic, technological and bureaucratic factors revealed in the case studies. This alternative explanation lies in the growth of political, bureaucratic and economic commitments quite apart from consideration of technical and economic merits. Despite its nurture in a hot-house of optimistic government policy and subsidy, nuclear power has not prospered. In short, the disappointing performance of nuclear power, nationally and internationally, demonstrates that zeal alone cannot overcome serious economic and technical deficiencies. Thus, international cooperative setbacks were an early manifestation of domestic failures. If the economics and technology had been sounder, international cooperation might have fared better.

No definitive case can be made for the alternative explanation, and Nau is admirably thoughtful in stating the limits of his own hypothesis. But further theoretical progress depends upon integrating technological, economic and political factors in an even-handed manner. Nau's provocative book provides much of the necessary evidence and valuable insights into the theoretical possibilities.

PETER F. COWHEY

University of California, San Diego

Race to Pearl Harbor: The Failure of the Second London Naval Conference and the Onset of World War II. By Stephen E. Pelz. (Cambridge, Mass.: Harvard University Press, 1974. Pp. 268. $17.50.)

Anyone who has studied the history of twentieth-century Japan knows how large a role the imperial army played in the 1930s in the approach of war in the Pacific. However, the responsibility of the Japanese navy, which has generally been portrayed as a moderating influence, has received much less attention—at least in the United States. *Race to Pearl Harbor* helps to correct this deficiency in the literature. It is an important study not only for this reason but also because of the author's wide-ranging use of Japanese, American, and British sources. Pelz deals in interlocking sections with the naval disarmament question as it was seen by the civil and military authorities in Tokyo, Washington, and London. In tight, lucid prose he portrays the personalities of the decision makers in each capital and the political, economic, and strategic pressures that shaped their responses to the problems confronting them. Pelz provides detailed enough historical background to bring the reasons for the failure of the Second London Naval Conference into sharp focus.

In Washington in the early 1920s the representatives of the leading naval powers had attempted to devise a system that would provide security for each of the participants in a naval disarmament treaty which was part of a larger settlement of Far Eastern issues. The failure of the signatories to deal adequately with the naval disarmament problem brought the same powers to London in 1930 when new arrangements were made. Theoretically, the Americans, the British, and the Japanese thus purged themselves of the need to compete, but in actuality the virus proved to be only dormant. Disarmament was popular with most of the public and with some of the politicians in all three countries, but naval officers were unhappy with the compromises that had been made. The seizure of Manchuria by the Kwantung army in 1931 marked the beginning of a decade during which a "hard line" group of Japanese admirals worked assiduously to build a fleet that would permit their nation to expand into China and Southeast Asia. The proponents of this policy were prepared to pursue this goal even if it ultimately meant war with the United States and Britain. Japanese army and navy leaders, working sometimes at cross purposes but at other times in concert, were more than a match for a succession of premiers and foreign ministers who held office in the 1930s. In Washington and London, a determination to reject the Japanese blueprint for a New Order in East Asia slowly gathered strength. However, because of domestic political and economic restraints Franklin D. Roosevelt was unable to respond in good season to the Japanese naval threat. In London, Neville Chamberlain repeatedly cited the shaky economic situation as the reason why a British naval building program had to be either postponed or trimmed to levels inadequate to guarantee the defense of the British Empire. In contrast, as early as the end of 1940, the Japanese navy was committed in principle to advancing into the western Pacific at some opportune moment during the fall of 1941, and was already mobilizing for war. Japanese naval leaders knew that they would have to act before the belated American building program produced an opponent capable of denying them the chance to pursue the ambitions they shared with their army colleagues.

Pelz concludes that the treaty structure of the 1920s and early 1930s went to pieces not because it threatened Japan or was discriminatory but because American and British leaders assumed that the Japanese were satisfied with their position in the world. However, the reaction of the Western democracies to Japanese initiatives in Asia produced a widespread feeling in Japan that peaceful economic advances were no longer an available option. The United States' and Britain's failure to maintain the military strength needed to support their political and strategic decisions encouraged Japan's military and naval authorities to draw over-optimistic conclusions about their ability to create a New Order in Greater East Asia by force. *Race to Pearl Harbor* is essential to understanding the naval aspects of the approach of war in the Pacific.

R. J. C. BUTOW

University of Washington

Human Behavior and World Politics: An Introduction to International Relations. By Ralph Pettman. (New York: St. Martin's Press, 1975. Pp. viii + 343. $12.95, cloth; $6.95, paper.)

Ralph Pettman, a Research Fellow at the Australian National University, has written an unusual book based on two beliefs he holds regarding the study of international relations. First, he is distressed by the theoretical incoherence that marks research on world politics, a view that is probably shared by nearly everyone else who has ever been close to the field. Second, he is convinced that this painful lack of coherence can best be remedied by addressing the whole of human behavior rather than by confronting international/transnational relations apart from other domains. Now, while the goal of a general theory of human behavior is also widely embraced, relatively few political scientists devote their energies to its direct pursuit because, in all probability, they do not share Pettman's view that a holistic approach is the most promising. Thus, the novel character of this book stems from the author's commitment to the virtues of grand theorizing.

From this commitment emerge two purposes for the book. First, Pettman hopes to show what constitutes the broader intellectual domain of potential, even if also peripheral, importance to the "core issues" of international relations. Second, he seeks to indicate links between periphery and core. In his words, the book

> attempts to organise a wide variety of ideas that these might be brought to bear in a comparatively novel way upon the study of global affairs . . . and makes a sustained attempt to break down the barriers that continue to prevent the inter-disciplinary understanding of politics at the most comprehensive of levels (pp. vii-viii).

Given these two ambitions, it may come as no surprise that the book is only partially successful.

The text is organized into three parts. In the first four chapters, Pettman considers the context in which international relations is studied. He begins with an overview of the academic heritage of science, with attention to science as substance, method, and aim. Discussion quickly turns to the merits and problems of behaviorism, followed, in turn, by a chapter on method and "theory" amply illustrated by the widely-known literature on elite decision making and the politics of linkages between domestic and foreign policy. Thereafter come extended reminders that human behavior is unique in evolutionary terms, that mankind's temporal and spatial existence is patterned, and that human necessity, self-awareness, and freedom may bear on the visible signals of interpersonal (and thus international) relations.

The second section turns from the existential context of science and politics to illustrations of the contributions to world politics scholarship offered by peripheral disciplines. The finite ecological limits to behavior are first to be identified. "Biopolitics" occupy the following pages, first with attention to studies of the human brain and then with regard to the behavior of other animals.

"Psychopolitics" completes the tour around the perimeter of international relations. It is likely that most readers will already be acquainted with at least some share of this field since images, personality traits, and public opinion are already rather well ensconced in what most regard as conventional world politics studies.

It may surprise the reader that, somewhere between the periphery and the core of international relations, Pettman fails to consider the potential of simulations and game theory. Moreover, he rules them out for the most curious of reasons—their findings "explain little about world politics" (p. 225, note 62; p. 240). This seems a bit illogical; the same is at least equally true of biopolitics, cybernetics, and the other "peripheral" fields that are reviewed at some length. The more serious consequence of such omissions, however, is that experiments with crisis decision making, chicken, and prisoner's dilemma games illustrate specific efforts to bridge the gap between the periphery of psychopolitics and the core of international relations study. Surely these investigations would serve the author's second mission, that of transcending conventional distinctions among academic disciplines.

The book concludes with two chapters that attempt to indicate the relevance of peripheral topics to the core issues of international conflict and integration. It is in these pages, then, that the missing links come to frustrate the author's second objective. Because (a) he cannot forge the necessary links himself, and (b) he does not attempt to capitalize on some of the few that may already exist, the "inter-disciplinary understanding of politics" he seeks must continue to rest on analogies and other heuristically suggestive constructs; the barriers remain.

Nevertheless, in fairness it should be added that this book does offer the reader succinct summaries and critiques of several types of behavioral and analytic research in international relations. More importantly, it contributes a useful digest of a wide sampling of disciplines and leaves one mindful of general perspectives on human behavior. Having thereby fulfilled its first objective, this volume provides in one place a compendium of literature that is otherwise widely scattered. Consequently, it may be of use in advanced undergraduate and graduate courses.

NEIL R. RICHARDSON

University of Texas, Austin

Understanding Conflict and War, Volume I: The Dynamic Psychological Field. By Rudolph J. Rummel. (New York: Halsted Press, 1975. Pp. 342. Price not listed.)

This is Rummel's attempt to lay the metaphysical bases for a "unified speculative, theoretical-empirical analysis of violence and war" (p. 5). Those who see the title and expect a discussion of the causes of war, and those who notice the author's name and therefore expect to find extensive analyses of empirical data, will be disappointed. Every time the subject of war comes up, it is discussed only briefly. Furthermore, no original empirical analyses are presented.

These are not disparaging remarks. They are meant only to forewarn the potential reader of this volume. Hopefully, the less surprised reader will be less likely to put the book down after the first prolix page. For the book deserves to be read. It deals with fundamental questions concerning the nature of reality and mankind which Rummel feels have been neglected by modern social scientists.

In order to deal with these fundamental questions, Rummel develops a world view, utilizing field theory, which he calls "intentional humanism." This view asserts that human behavior is a function of dispositions weighted by human beings' expectations and personality weighted by the situation they perceive. What they perceive is not what is actually "out there"; rather the process of perception is a dynamic conflict between the attempts of an outer world to impose actually on them, and their efforts to transform this actuality into a self-centered perspective. Finally, human beings do have a free will, and the preeminent goal toward which this will directs behavior is self-actualization.

If readers feel they fully understand the previous paragraph, perhaps they are not paying attention. Obviously, this book covers too many profound philosophical issues to be dealt with in a brief review. Therefore, let us discuss only two of the several controversies that will swirl around this book.

First, do human beings have free will? Rummel insists that modern social science is based on a deterministic philosophy. His alternative is basically Kantian. On the level of phenomena, human behavior is determined. But on the level of "things-in-themselves," human beings have a free will. This is crucial, because if human behavior is determined, war is also determined, and unavoidable. But, according to Rummel, since mankind has a free will (or at least it does not contradict reason to believe so), it is possible to eliminate war.

It seems to this reviewer that even if Rummel's modified Kantian argument is accepted, it does not clearly provide an escape from the dilemma which concerns the author. Human beings may have free will on the level of "things-in-themselves," but surely we have to deal with war as a phenomenon. By definition, we can not perceive, or deal with, war as a "thing-in-itself."

Must we argue, then, that human beings are also free on the level of phenomena? It is interesting to recall that behaviorists such as Skinner claim that human beings cannot solve their most serious social problems unless they give up their rigid adherence to the "free will" idea. Rummel, obviously, believes that he must have a free will to solve those problems. The logic of both arguments is quite clear. If humanity is truly free, then it is essentially unpredictable, and we will never be able to understand or control human behavior, or war. If human beings are truly determined, then they are victims of external causes, and war is unavoidable.

Both arguments make sense as far as they go, but it seems to this reviewer that Skinner, Rummel and any other social scientist who wants to produce beneficial research must ultimately argue that human behavior is determined enough to be predictable (and therefore understandable and controllable), but sufficiently free (or undetermined) to allow intelligent intervention in the course of human events. Rummel may well have drawn a nonexistent distinction between his position on free will versus determinism, and the position of most other social scientists. The author's adherence to the Kantian argument is distinctive, but is not clearly helpful. Stripped of its Kantianism, Rummel's position is that human behavior is partly determined, and partly free, a position which I *believe* is prevalent in modern social science.

In contrast, Rummel's preference for teleological explanations of human behavior which emphasize such "internal states" as subjective perspectives, expectations, and dispositions probably does set him apart from most contemporary social scientists. This particular aspect of intentional humanism may have unfortunate consequences. Because these perspectives, expectations, and dispositions are basically invisible, they are difficult to deal with scientifically. (I am not borrowing the Skinnerian argument here. Skinner seems unaware of two crucial considerations: (1) Not all concepts in a theory need be operational. It is only necessary that the theory eventually produce empirically testable statements. (2) "Invisible internal states" do leave physical traces, if it is possible to administer psychological tests to the persons we are studying. But this seems especially unlikely in the discipline of international relations.) Still, if such factors are crucial, we should make the effort. However, would it not be more fruitful to begin with the basic theoretical assumption that similar people (similar in terms of visible background characteristics) under similar circumstances will behave in a similar fashion? That assumption would imply that, given measurable similarities in people and their circumstances, the perspectives, expectations, and dispositions of those people will also be similar. This being the case, behavioral outcomes would be predictable given *only* direct knowledge of the similarities in the people and circumstances, and we could safely ignore invisible internal states. (At least in our *empirical* analysis. They could still be included in a theory.)

I suspect that Rummel's reply to this argument would be that he does not ignore visible environmental factors, and that social science which has de-emphasized subjective perspectives, expectations, and dispositions has not been spectacularly successful. He would be right on both counts, but I nevertheless feel that he overemphasizes subjective factors, and I still have doubts about his proposed solution to the problems which face social science. (Conveniently, space does not permit discussion of my proposed solutions to those problems.) In any case, criticism *or* praise of this initial volume is somewhat premature. The ultimate value of this book will be determined to an important extent by the volumes to come. The first volume certainly reveals a cultivated, intelligent, and probing mind at work. The succeeding volumes should allow us to decide whether *Understanding Conflict and War* is praiseworthy as well as very ambitious, or merely pretentious.

JAMES LEE RAY

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Le pétrole à l'heure arabe. By Nicolas Sarkis. Introduced by Eric Laurent. (Paris: Editions Stock, 1975. Pp. 320. Price not listed.)

Nicolas Sarkis (Sorbonne Economics doctorate), the well-known consultant for the Organization of Petroleum Exporting Countries (OPEC), seeks to provide the Arab view of petroleum principally in the context of the major oil producers and the Western and Japanese industrial consumers. In a well articulated, wide-ranging question and answer format carefully buttressed by useful data, Sarkis examines the future availability and usage of world oil, that fast-depleting, most important energy commodity.

Oil has been the means, he asserts, to industrial world development. It must also be the means to development of the Third World, of which OPEC members form a part, before it is substantially exhausted. Cooperation is needed between producer and consumer, but even more necessary is an understanding of the Arab position. For instance, too often Westerners criticize "Arab" price hikes which are in fact OPEC decisions. It bears repeating that Indonesia, Iran, Nigeria and Venezuela are development-oriented, revenue-hungry OPEC members, clearly non-Arab, who favor high prices in contrast to Saudi Arabia, the most important Arab producer and leading world exporter.

The October 1973 Arab-Israeli conflict initiated a politically-inspired Arab boycott. But it was the ensuing fourfold OPEC price hikes that brought oil prices in line with other post World War II commodity price rises. Much has been made of the newly rich Arabs. In fact the oil revenues as a percentage of industrial world GNP are modest: the bulk of the wealth remains with the industrial world. For example, the estimated $60 billion in 1975 Arab oil revenues approximates the budgetary expenditures of France alone and but 4.3 percent of U.S. national income for that year. Most independent observers would agree with Sarkis that the oil price hikes, while disagreeably abrupt, were not the major cause of inflation which was already a problem for the key oil importers. And they might admit that Western nations and Japan, while they had to pay more for oil, also managed to export inflation through high priced finished products and foodstuffs. Oil income grudgingly provided by the users is essential for development of Arab and other Third World countries.

Arab inability to absorb oil revenues—the much touted "surplus"—attests to the lack of modern infrastructure, not, affirms Sarkis, to the lack of needed development. Arab goals are to "transform their oil and their oil revenues into factories, dams, agricultural enterprises, into schools.... It is all the more urgent as petroleum is a non-renewable wealth" (p. 35). It is a question of goal achievement in three decades.

At a time when Arab natural resources provide the means to development, Sarkis is concerned that the foreign companies have been flaring wastefully most of the gas associated with oil for years in the Arab countries—some 110 billion cubic meters in 1973 worth $6 billion. In this reviewer's opinion, such practices were valid enough in terms of economics if not long-term energy needs and were found even in the United States. In brief, the companies may be charted with shortsightedness but not with perfidy.

The 1973 Arab-Israeli war highlighted, but did not cause, the energy crisis. Two American physicists, Lawrence Rocks and Richard P. Runyon, would corroborate the argument that power shortages have been looming for years but became recognizable at the beginning of the 1970s (*The Energy Crisis*, 1972). America has been a net importer of oil, its chief energy source, for almost 30 years.

The oil embargo was not a new idea, Sarkis goes on. There are ample Western precedents for the use of embargo as an instrument of foreign policy. While this reviewer understands Arab anger at Israeli occupation and (now diminished) oil company privileges, he must disagree that the use of oil as a political weapon was indicative of a higher morality than, say, the U.S. "quarantine" in the Cuban Missile Crisis. In a world of sovereign nations, the pursuit of security and freedom from exogenous control are all "moral" goals, even if the attempts to achieve them result in violations of international law.

The War sensitized the industrial West and Japan to their dependence on Middle Eastern and—increasingly—Arab oil. Previous efforts to assert producer country sovereignty such as the 1951 Iranian nationalization were aborted. Sarkis states that the major oil companies, most of them American, continued to subsidize Western and Japanese industrial growth with cheap oil. Sarkis is right to assert that the Western charge that present prices are unjustifiably high because Arab production costs are low fails to recognize that low production costs do not take into account the concommitant depletion of resource capital. If the *total cost* of this diminishing asset is taken into account the picture is very different.

Ironically, America has a stake in high prices as they make viable our costly development of dwindling oil reserves and alternative energy sources. High prices give the U.S., which is still relatively energy-rich, an advantage over relatively energy-poor Western Europe and energy-resource poor Japan, its commercial rivals.

Higher prices did not appear until the OPEC nations began to assert control over their own resources and dramatically increased their share of the take. But Sarkis warns that "this increase in absolute value of revenue per barrel masks in reality a strong diminution of real purchasing power in the receipts of the exporting countries" (p. 79). Successive devaluations of the dollar and the pound sterling have eroded the monetary gains of the OPEC members. Monetary stability is desperately needed. The much-feared massive transfer of wealth to the Arab world has not materialized. Much of it has made its way back to the West. The Arabs will develop greater absorptive capacity in time, however.

In all, this highly-respected, admittedly partisan petroleum economist provides an articulate, well-reasoned presentation of the Arab oil producer's position which is not fully understood in the West. He favors cooperation between producer and consumer with aid for the resource-poor lesser-developed countries. This idea strikes a rational note as energy and food self-sufficiency may prove to be even more elusive goals in the international system than weapon-derived security. His demand for relations on the basis of full equality between producer and consumer has met with more response in France than the United States, but this may change. As is well known, American consumption of imported, increasingly Arab, oil has increased. The transition to a non-oil and non-gas economy requires Arab cooperation which, it should be recognized, has been largely forthcoming.

Continued cooperation requires stability. There are many "rights" in the Middle East: Arab recovery of occupied territory, a Palestinian home, and Israeli security. Will the West's need for oil and the Arab's need for industrial goods, food and technology pave the way for an equitable settlement? Resumption of hostility is costly to all sides. It is difficult to imagine, says Sarkis, oil not being used again as a political weapon in the event that hostilities are resumed. In such an eventuality, says this Syrian-born Christian with some spirit, "The Arab governments have not only the right but also the duty to use their oil as a 'political weapon' " (p. 56).

Since this book was written, Russia has passed the United States as the world's largest oil producer. But America remains vulnerable as the world's largest consumer. Perhaps by the end of the century the People's Republic of China will lead the world in oil production. At this point, however, as American production declines and imports increase with our scant regard for conservation and more efficient energy utilization, it might well be the Arab's "hour."

JOHN G. MERRIAM

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The Anschluss Question in the Weimar Era: A Study of Nationalism in Germany and Austria, 1918–32. By Stanley Suval. (Baltimore: The Johns Hopkins University Press, 1974. Pp. 240. $12.50.)

It is one of the major ironies on the international political scene of Europe between the wars that the Anschluss, firmly denied by France and her allies to Weimar Germany and its Austrian sister republic, finally came about in the form of Nazi annexation of helpless Austria with the Allies looking on, or rather looking away. This fact above all has prompted historians to investigate the earlier phases of the Anschluss question. Stanley Suval's book is one of a number on the subject which have recently appeared. The author, approaching the Anschluss movement as a "model for the study of nationalism" emphasizes its "pluralistic" nature. Below the surface of patriotic purpose which agitated the Austrians in particular after the deluge of 1918, there were distinct strains within the Anschluss movement: democratic and antidemocratic, middle class and socialist, federalist and centralist. Even in unison these could not surmount the tight prohibitions set by Versailles and St. Germain (1919) and Geneva (1922). The conclusion, then, that "frustration summarizes the Anschluss movement as no other word can" is a truism. A more challenging task would have been to trace the change of climate after 1933 from "frustration" to aggression.

Even within the limits set by the author the book is wholly inadequate. The research is incomplete and sloppy, and the volume contains innumerable mistakes and misspellings. The boxes and microfilms from the Vienna Haus-Hof-und Staatsarchiv and the Washington National Archives are haphazardly selected and many important ones are missing; furthermore, the vital Political Archive of the German Auswärtiges Amt was not consulted at all.

As for substance, the author might have taken his clue from a statement on the Anschluss generally attributed to the Austrian Chancellor Seipel: "Always talk about it and never think of it." This comment reflects the duplicity which characterized almost every statesman and politician when it came to the question of German-Austrian union. Without this key Suval is unable fully to understand the motives of the major figures involved.

The historical personality of Seipel is thoroughly misconstrued. To begin with, he was not "the only priest in the twentieth century" (p. xx) who was prime minister (what about Tiso of Slovakia?). But more grave yet is a flat misquotation from Seipel's printed diaries which sets the author off on the wrong course. In August 1928, one year after the catastrophic Viennese events of July 15, 1927, as Seipel saw his political work crumble, he wrote in his diary: "My task *after* the first ten years of political life: counterreformation. . . ." However, Suval transcribes this entry to read: "My task *in* my first ten years of political life was to maintain a counterreformation" (p. xx). No wonder therefore that Seipel should appear to him "intense and humorless." Much to the contrary, Seipel's venture into the world of politics represented a gamble on his part to reconcile the prerogatives of the Church with the reality of the secular state and also the tradition of empire with the reality of the small Austrian state. He enjoyed the game of politics, and the deviousness with which he often proceeded was his way of humoring a world that did not face up to his challenges.

For this reason, nothing that Seipel did was ever simple and direct. His stopover in Berlin in August 1922 in the course of his "great trip" to Prague, Berlin, and Verona was not devoted, as Suval asserts (p. 204) to proffering the plan for an Austro-German customs union; more than anything it was designed to stir up the powers about the condition of Austria. Neither can it be said that Seipel "privately supported" (p. 224) the proposed Customs Union with Germany in 1931; on the contrary, "toward the outside world" Seipel advocated a strong and united front, but he confided to his party friends that "the criticism should be reserved for later"—that is, in case the game was lost. Finally, Seipel was not forced out of office (p. 146) in April 1929; as in April 1924 when he first resigned, he himself chose the time for his resignation and it came like "lightning . . . out of blue sky" (*Neue Freie Presse*, April 4, 1929, morning ed.).

For the rest, I might set straight some of the blatant factual mistakes in the book: Adenauer was Lord Mayor (not Mayor; p. 37) of Cologne; Sir Robert Vansittart (not Vansitart; p. 154) was Permanent Under-Secretary of State (not Secretary of State); Oswald (not Otto; p. 197) Redlich; Ernst K. Winter became Vice-mayor of Vienna under Dollfuss (not Mayor of Vienna under Schuschnigg; p. 199).

On such foundations, what scholarly results can be expected? The book moreover is poorly written and inconclusive. Might one not explain the irony mentioned at the beginning of this review by the fact that when the Anschluss was consummated in 1938, France had given pride of place in the European power structure to Great Britain, and that the latter in earlier years had always taken a more lenient position than the French on the proposition of a German-Austrian union?

KLEMENS VON KLEMPERER

Smith College

Snow Job: Canada, the United States and Vietnam (1954 to 1973). By Charles Taylor. (Toronto: Anansi Publishers, 1974. Pp. ix + 209. $8.50, cloth; $3.95, paper.)

At the end of this book, the author and journalist Charles Taylor deplores the tendency of Canadians to sermonize; unfortunately, his own book might be taken as an example of this fault.

This book passes judgment throughout on the behavior toward Vietnam on the past of Canadian prime ministers, secretaries of state for external affairs, diplomats, and military observers. Repeatedly, he dwells on how badly they behaved and how well they would have behaved if they had followed the right ways, i.e., the way defined by Charles Taylor, some two to twenty years after the events took place.

Charles Taylor's thesis is that Canada was an "accomplice" of the United States in Vietnam from 1954 to 1972. This complicity took, according to Taylor, several forms: Canada approved the bombing of North Vietnam; the Canadian personnel of the International Control Commission (ICC) established by the Geneva Conference of 1954 were used by the United States as messengers to threaten North Vietnam and to collect military information; Canada sold arms to the United States which were partly used in Vietnam; Canada became a member of the International Commission of Control and Supervision (ICCS) set up at the Paris Conference in 1972 only to facilitate the U.S. retreat from Vietnam. According to Taylor, Canada should have withdrawn from the ICC in 1962 when it was evident that both parties had violated the Geneva agreement and never should have accepted membership in the ICCS.

The so-called approval of the bombing of North Vietnam, which constitutes the "scandalous revelation" of the book and which seems to have motivated its publication, is based on nothing more than a conversation between Lester B. Pearson and Lyndon Johnson. This conversation apparently took place in a New York hotel on 28 May 1964.

Unfortunately, the only record we have of this conversation comes from a cable to American Ambassador Lodge in Saigon, signed by acting Secretary of State George Ball, and published in the Pentagon Papers in 1973. Mr. Ball based the cable on a report about this meeting written by McGeorge Bundy. In other words, it is based on an interpretation by a witness of what Pearson actually said, subsequently incorporated into the minutes of the meeting.

When this cable was published, Pearson was dead. In his personal papers there is nothing about this meeting. Such an attitude, if it was in fact adopted by Pearson, would have been in complete contradiction with all his other actions and pronouncements with respect to the American air strikes in Vietnam. Taylor himself describes how, shortly after Pearson's government came to power in 1963, Secretary of State for External Affairs Paul Martin conveyed to Dean Rusk and Robert McNamara Canada's doubts about American policy in Vietnam, and argued in favor of a negotiated settlement. But the main piece of evidence regarding Pearson's real attitude is his famous Temple University speech of April 1965 when, without giving prior notice to President Johnson, Pearson urged the United States to initiate a pause in the bombing of North Vietnam. Taylor writes that when Pearson went to lunch with Johnson in Camp David the following day, he was subjected "to an arm-swinging, lapel-grabbing diatribe that stopped just short of physical violence" (p. 38). This hardly supports Taylor's theory that Pearson at one time expressed approval of the bombing, which could only have taken place under the most unusual of circumstances, if at all.

Concerning the collection of military information on North Vietnam for the United States by Canadian representatives on the ICC, Taylor is able to cite only one case, that of a brigadier whose information was subsequently disavowed by Canada in 1958–59, and an American officer's allegation that he saw information which had been supplied by a Canadian. The repeated use of the word "complicity" is hardly justified by the number of facts presented to demonstrate it.

Regarding the role of the "messenger" (p. 63), Taylor is simply ridiculous; he appears to miscalculate completely the legitimate role of the diplomat Seaborn, who played the role of "dispassionate intermediary." (Lyndon Johnson's *Memoirs* cited by Taylor, p. 63). Taylor considers that "the Canadian government continued to acquiesce in U.S. policy by allowing Seaborn to continue in his role as intermediary" (p. 83). But Seaborn's missions—like those of Ronning, later, and those by people of other nationalities—were *welcomed* by North Vietnam as constructive efforts to stop this disastrous conflict. The only reason we can see for Taylor's objection to Canadian diplomatic actions is Pearson's supposed acceptance of bombing, referred to above.

Canada became a member of both the ICC and the ICCS, not because of America's sole and unique insistence, but at the request of Peking, Hanoi, and Washington. Taylor himself tells us that Chou-en-Lai proposed Canada at the Geneva Conference in 1954. In 1973 both Hanoi and Peking were in favor of the Canadian presence on the ICCS. It seems strange that India, which is the model of behavior for Taylor on the ICC, was unanimously rejected as a member of the 1973 ICCS. Aware of the difficulties of operating without clear authority to report on the violations of the 1954 agreement, Canada nevertheless, on the insistence of Hanoi, Peking, and Washington, agreed to become a temporary member of the ICCS during the essential time at the end of the fighting, during the exchange of prisoners and the retreat of American troops. It subsequently withdrew from the ICCS for exactly the reasons which Taylor considers should have motivated a Canadian withdrawal from the ICC. The difference is that Canada committed itself without reserve in 1954 and, having learned from experience, only for a limited period in 1973.

It is a pity that Taylor is so much less attentive to the really serious weakness of Canadian policy in the Vietnam war: the question of arms sales. Canada sells some $300 million worth of arms and ammunition per year to the United States under the Defense Production Sharing Agreement of 1959. During the Vietnam war, an unknown portion of these armaments were used in Vietnam. It is clear that selling arms and playing truce keeper and mediator are in keen contradiction. The Canadian government's defense was that terminating the Production Sharing Agreement would have been interpreted as withdrawal from NORAD and the Atlantic Alliance However, if Canada had put an embargo on arms clearly destined for Vietnam—e.g., napalm—while reaffirming its commitment to NORAD and the Atlantic Alliance, the United States would probably have accepted the situation—naturally, under protest.

In conclusion, this book is full of allegations and preachings but fails to insist on the main weakness of Canadian policy toward the Vietnam war. It would have profited from a much greater attention to analysis and a lesser preoccupation with ready-made moral judgments.

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An Economic Background to Munich: International Business and Czechoslovakia 1918–1938. By Alice Teichova. (New York: Cambridge University Press, 1974. Pp. xx + 422. $27.50.)

The purpose of this book by Alice Teichova is unclear. It consists in the main of an exposition and analysis, first, of foreign investment in Czechoslovakia before 1938, particularly in the central economic activities of mining and metallurgy, mechanical and electrical engineering, chemicals and banking, and secondly of Czech participation in international cartelization in the three industrial fields. The exposition contains much information, and the analysis is searching and often subtle. But Teichova concludes her introduction: "In this area [Central and South Eastern Europe] international political developments before Munich influenced and in turn were affected by economic factors and these relationships are analysed in this book" (p. xx). From this the reader looks forward eagerly to an attempt to grapple with the formidable methodological problems of linking observable economic developments with observable political occurrences. No such attempt is made—indeed the author seems only intermittently to be aware of the hazards of deducing political consequences from economic facts or relationships. Thus she writes at one point: "The new states . . . although politically and formally independent, were in reality encompassed by a net of financial and diplomatic relations reducing them to various degrees of dependency: the world's largest combines and the great banks increasingly asserted their influence on their economies; their politics were, in effect, subordinated to the political power systems of the Great Powers" (p. 15); but elsewhere "the effect of these powerful cartels on the economic policy of their states, and even on the relations between states, cannot be doubted, although it is difficult to measure" (p. 322). On p. 102 she asserts, "Eugène Schneider exerted considerable influence on French policy as a leading representative of the powerful Comité des Forges," giving no evidence for the assertion, and citing Maurice Thorez as her authority, while on p. 125 we find her writing "the Mannesmann-Röhrenwerke A.G., Düsseldorf . . . was one of the leading industrial combines. . . . It is *therefore* [my italics] not particularly surprising that we find the Mannesmann group supporting the programme of the NSDAP . . . and contributing to the funds of the Nazi Party."

It is of course not my intention to argue that all, or any, of these statements are false. My observation is that the difficulty of deducing them from the economic data that are presented is not understood. Therefore, from the point of view of a student of political science or of international relations the book offers little of direct assistance. Its indirect assistance is considerable however. If the political addenda are shorn away there is left a most interesting and suggestive compendium of information about levels of types of foreign investment in the inter-war period, about relative shares of different states in various economic activities, about the intricate relationships among large enterprises in different states, and to a much smaller extent about movements and trends in economic relationships. Teichova has searched through extensive sources and has fitted together disparate material with considerable skill; the picture which emerges is clear. One might regret that while she has statistics for successive years for, for example, many of the most important components of Czechoslovakia's foreign trade, most of her data on foreign investment in Czech industry is derived in each case only from a single year, and it is accordingly difficult to appreciate trends. Therefore arguments remain inconclusive regarding for instance—the extent to which British and French enterprise was or was not willing before Munich to allow increasing German penetration. But the detective work required to assemble the data is recognizably difficult, and Teichova rather deserves congratulation for what she has achieved than criticism for not having been able to do more.

In sum, then, this is a book for the international political scientist interested in the inter-war period to have on the shelf, constantly to refer to for its suggestive data, but to be treated with caution when it strays into political deduction.

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Inside Foreign Aid. By Judith Tendler. (Baltimore: Johns Hopkins University Press, 1975. Pp. 140. $9.95.)

This is a sophisticated book by a shrewd observer of organizational behavior. After three years of work in Latin America as an economist for the United States Agency for International Development (AID), Judith Tendler gives us an insider's view of the middle-level bureaucratic interaction among AID employees in the field, the direct beneficiaries of their aid, whether they be host governments or local companies, and the development officialdom in Washington. Tendler eschews dry statistics and development jargon, focusing instead on the political constraints, organizational imperatives, and career incentives involved in dispensing development funds. The result is one of the most perceptive and succinct criticisms of foreign aid that I have yet encountered.

The thesis of the book is this: the political and bureaucratic imperatives of American foreign aid operate upon the beneficiaries and organizational dispensers of funds alike so as invariably to encourage large capital projects with heavy doses of sophisticated technology and extensive import requirements. In foreign aid, it is the big and flashy capital projects that are "beautiful," not the small, carefully tailored, labor-intensive ones.

The political imperative derives from the tied nature of American aid. In order to sell foreign aid to the Congress, successive Administrations have had to tie aid by two means—first, by restricting the use of aid funds to pay for only the import, not the local, costs of projects; second, by stipulating that such imports come only from the United States, not from any other developed nation. Despite some recent relaxation of these two requirements, more than 80 percent of total AID funds are still spent in the United States. The reason for these policies is clear: like the State Department, AID began with a weak bureaucratic position in Washington. It had few, if any, grass-roots domestic supporters for its programs. Unlike State, however, AID has been able to develop and exploit—if only in a limited fashion—the small domestic constituency that tied aid has produced. But there has been a price to pay. Personnel in the AID field mission know that the larger the project, the greater the capital outlay, and the more extensive the requirements for the use of American technology, the more likely will it be that the project will be funded by Washington. Similarly, the local beneficiary of the project knows that, unless he goes along with these requirements, he may get no American financing for his project. Because American funds, even if spent only on import costs, often make the difference between having and not having a project, the local beneficiary naturally prefers a project tailored to suit the American benefactor to none at all. The costs of building an American constituency by tying AID's aid are thus clear: in the competition for scarce funds, it is those projects which most fully benefit American exporters, not those which would most fully benefit local, host country entrepreneurs, that stand the best chance of being funded. AID's specific programs have become hostage to the domestic constituency that was judged necessary for AID's existence in the first place.

The bureaucratic imperative in America's foreign aid program is more subtle than the political one, but equally as potent in its effects. The standard criterion for success, both for AID as a whole and for the personnel working within it, is how much money has been "moved." Individual careers are made and overall institutional performance judged, not solely or even primarily by the quality of the funded programs, but by how much money has been spent. The pressure to spend favors large programs over small ones. Large projects are more cost-effective than small ones: the larger the given project, the smaller the percentage of total costs that AID's overhead and administrative expenses absorb. Large projects also move money more efficiently: it is easier to spend a given annual allocation by funding a few large projects than by funding a large number of small ones. Large projects thus drive out small ones. But, again, the costs of this quantitative approach to measuring success are clear. Host governments and local entrepreneurs are less able to generate proposals for the big capital projects than they are for the smaller, more labor intensive ones. That, in turn, requires AID personnel in the field to become the generators of the demand for the funds that they themselves dispense. Because field personnel know best which projects Washington will approve of, they become intimate participants in the proposal-making process. In short, in the pressure to spend funds and build careers, Americans once again take over.

The heart of Tendler's critique of America's foreign aid is, not that such aid is totally useless, but that it is suboptional. Local entrepreneurs will always derive some benefit from a big capital project; local talent will always gain some expertise in development planning. But the aid program is not promoting its objectives as fully as it otherwise could. If the development of local industry, local entrepreneurialship, and local planning expertise are to be the aims of AID, then American exporters and American experts must assume a lower profile. But, as Tendler says, "my solution is simple only in terms of its logic" (p. 110). She has given us a penetrating analysis of the shortcomings of America's foreign aid, but her very analysis lays bare the potency of the political and bureaucratic constraints and their obdurate resistance to amelioration. Accurate analysis, however, is the key to better policy, even if such analysis makes one pessimistic about the prospects for change.

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Europe and the World: The External Relations of the Common Market. Edited by Kenneth J. Twitchett. (New York: St. Martin's Press, 1976. Pp. xviii + 210. $16.95.)

The volume consists of seven essays by different authors including one by the editor himself. Twitchett's essay introduces the subject and discusses the bases for the external relations competences found in the EEC Treaty. The second piece is by Roger Morgan who examines the transatlantic relationship between the European Community (EC) and the U.S. John Pinder, in the third essay, looks at the problems of the Community relations with "state-trading" countries, followed by Avi Shlaim's analysis of the relationships with the Mediterranean countries. Carol Cosgrove-Twitchett contributes two essays, the first examining the Community's association policy and the second dealing with the possibilities of elaborating a comprehensive development policy. The final piece, by F. S. Northedge, focuses on British foreign policy in a Community context.

Twitchett's essay raises two conceptual questions. Does the Community's international activity constitute "foreign policy" or should it be seen merely as "external relations"? The author opts for the latter, but is this really an "either-or" issue? External relations refer to the conditions which every state or intergovernmental organization (IGO) has with other governmental or nongovernmental entities. But the dynamics of and changes in these relations flow from policy actions and the Community as a collectivity of nine member states formulates and implements certain parts of foreign economic policy through the EC legal and institutional framework. Hence, it is entirely proper to speak of external and indeed foreign policies of the Community, especially since certain policy instruments such as associations are available to the member states exclusively through the Community framework.

The other somewhat questionable concept put forth by Twitchett and followed throughout the book by the other authors is the definition of the Community as a "civilian" power in world affairs. The term suggests that the Community exercises influences by "commerce and diplomacy, not traditional military strength" (p. 2). However, do not the "military" powers, especially the U.S., also exercise international influences more often than not through economic strength, and do not the Big Three in the Community also dispose of a considerable military power that has been influential, particularly when it is linked to sales of military hardware in different parts of the world?

Twitchett's description of the Davignon machinery for the foreign policy coordination of the member states and of the Community's increasing relations with regional IGOs is good. His discussion of EC aid to nonassociates is interesting but his figures do not fully conform with the data in chapter 6 (p. 168).

In his essay on the transatlantic relationship, Morgan points to patterns of American foreign policies that have produced recurring frictions. The overambitious diplomatic schemes of Dulles in 1954, Kennedy in 1962, and Kissinger in 1973 are examples as are the disregard of European interests by the United States during the Suez crisis in 1956, the Connally monetary action in 1971, and the Middle East war in 1973. On the other hand, Morgan's arguments against criticism by the U.S., Canada, and Japan regarding the discriminatory nature of EC preferential trade agreements with a number of Third World countries are weak. Indeed, the merit of this criticism has been recognized by another contributor to the volume, Avi Shlaim, who deplores that the Community is undermining the GATT principles through the introduction of a multiple trade discrimination system, which contravenes Article I of GATT. This system extends the geographical limits of the Community and has justified suspicions that the EC is seeking to establish a new bloc in world trade through what Shlaim calls an "enclosure strategy" (p. 111).

Pinder's examination of EC relations with Communist countries reveals deep pessimism about the role of the Community institutions in evolving a proper policy acceptable both to the member states reluctant to lose their sovereign foreign policy prerogatives as well as to Eastern Europe. He states correctly that only

> if the Community were to have all the instruments needed for industrial co-operation, commodity agreements and long-term contracts, and financial co-operation, in addition to the instruments of commercial policy, there would be great scope for the development of its economic relations with the state trading countries, and some chance of establishing a better equilibrium in relations with the Soviet Union (p. 75).

The most profound evaluation of EC external relations is found in the Avi Shlaim essay. It not only covers the Mediterranean basin per se, but also deals with related aspects of Community policy toward developing countries. This reviewer does not accept the widely propounded argument that historical links and geographic proximity make the Mediterranean region an area of "special responsibility" for the Community, thereby perhaps creating a new kind of *mare nostrum*. Rather, the basic motivation for the Community's Mediterranean policy seems to be the establishment of a sphere of economic and perhaps political influence, despite the fact that, as Shlaim points out, the Community decision-making apparatus is not well equipped for the task of "empire building" (p. 115).

Shlaim states correctly that the maze of preferential agreements with the Mediterranean countries is likely to provide only a very limited stimulus to industrialization and growth of their exports. These countries must compete with the Lomé associates whose goods are admitted to EC member states in principle free of customs duties. Moreover, the generalized system of tariff preferences (GSPs) introduced by the Community, although far from being the panacea for the nonassociated Third World it was claimed to be, also reduces potential benefits for the Mediterranean countries. I agree with Shlaim's contention that "the most promising approach . . . would be:

> to contribute to the economic development of the Mediterranean and other associates, not by the granting of preferential tariff rates, but by increased aid which would stimulate investment and boost industrial exports. This in turn would facilitate the extension of the GSP for the benefit of all the LDCs, thereby lending some credibility to the EEC's claims that it is not just a rich man's club, but a liberal and outward-looking body conscious of its responsibility towards the Third World (p. 120).

Although not reaching Shlaim's analytical depth, Carol Cosgrove-Twitchett's two essays offer very competent descriptions of the Lomé Convention and the struggle for a comprehensive Community development policy. Her generally realistic appraisal of the Community's policy efforts and prospects suggests a thorough understanding of the political forces within the complex EC system.

In the final essay Northedge points out that British foreign policy will benefit from EC membership by obtaining new resources for goal attainment. While Britain's foreign policy is not restricted by any legal obligation for policy coordination among the Nine, such coordination may be considered as convenient from time to time.

The British perspective is strongly emphasized, and the book is more oriented to EC relations with the Third World than with the advanced countries. As in many collections of essays by different authors, the volume suffers from overlaps, variations in style, and occasional conflicts of views. Nevertheless, the book is worthwhile reading and makes a definite contribution to our knowledge of the Community system.

WERNER J. FELD

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The Advisors: Oppenheimer, Teller, and the Superbomb. By Herbert York. (San Francisco, Calif.: W. H. Freeman, 1976. Pp. x + 175. $6.95.)

Herbert York presents an engaging argument that President Truman's 1950 decision to produce the H-bomb (overruling the recommendation of the Atomic Energy Commission's General Advisory Committee, chaired by J. Robert Oppenheimer) needlessly expanded the arms race and tensions of the Cold War. The book is meant to be a precise reexamination of the substance in this most important policy choice, but it also gives the reader vivid glimpses of the processes and personalities involved, and of the political tone and psychological background of the period. The book is extremely lively and well written. York is an unusually suitable author for this kind of work, an "insider" who was there at the time and knew all the people, but who is not thereby dissuaded from reading the documents and secondary accounts which have dealt with the same subject. "Insider accounts" are all too often based on nothing but first-hand experience; this is not the case here.

York's hypothetical possibility is that Americans thus indeed missed a chance to cool down the arms race, because we were too afraid of losing it. As with the 1945 use of the A-bomb (about which too much has been written), and the 1960 introduction of intercontinental missiles, the testing of H-bombs in the early 1950s inevitably sent messages, and conditioned expectations, in each direction. York makes a persuasive case that needlessly hostile messages were sent and that probably a needlessly awesome weapons stockpile was accumulated on both sides.

This is of course all speculative, as York would readily admit. Yet speculating about missed opportunities of the past may be the only way to avoid missing opportunities in the future. One sees at least a few striking analogies between the choices made more recently on MIRV or cruise-missiles and the earlier choice on the H-bomb. Why not let the other fellow catch up or pass us in redundant categories of weapons? Why not wait and see whether he might lose interest in the race?

The book presents a closely reasoned and documented analysis of evolution of the Soviet H-bomb program, showing quite clearly that the USSR was never ahead of the U.S. in the development of such weapons and never even threatening to be ahead. The 1949 report of the General Advisory Committee thus may have been quite correct in counseling President Truman not to fear any military disadvantage for the United States if it held back in moving to an H-bomb program. York's hypothetical "worst-case" for American restraint is presented on page 96, with the United States coming in a few months behind the USSR in H-bomb acquisition, *even if* the Russians had not been slowed down at all by the signals of American restraint.

Among other things, York in this book thus wishes to decisively rebut accounts that imply or state that the USSR actually "won the race to the H-bomb" (for example, Nuel Pharr Davis, *Lawrence and Oppenheimer*). Yet here the reviewer might begin to find a flaw in York's argument. We do not really know how the American government and public would have reacted if the Russians had seemed to be coming in first with an H-bomb. Despite the few books which York wishes to correct, most Americans have all along assumed that *we* won the H-bomb race. York may be forgetting this by becoming too absorbed in rebutting some misleading summaries of the race written by his fellow specialists. York assumes that the political costs of possibly being beaten by the USSR were worth risking; but the 1950–1956 years do not really show us how tense Americans might have become. A much better approximation might be found in the "missile gap" fears of 1957–1960, when the Soviet Sputnik clearly won the race into orbit. In terms of impact on arms budgets or other Cold War pathologies, more damage may have been done in "letting the Russians get ahead" than was suffered in the H-bomb case by "keeping them behind us." The Cuban Missile Crisis and 1000 Minutemen, and in many ways the Vietnam War and MIRV—these may be the missed opportunities of the race the Russians won. Compared to this, the missed opportunities of the H-bomb race may be harder to specify.

There is also at least one version of the myth that "the Russians beat us to the H-bomb" that York does not confront directly. As York makes clear (pp. 85–96), the first American H-device came before the Russian H-device, and the first U.S. prototype bombs were detonated in March of 1954, while the first Russian bomb was detonated in November of 1955. But the first Russian bomb was indeed test-detonated in being dropped from an airplane, while the first American test drop from an airplane came only in May of 1956. York's account does not mention this last fallback for a supposed Russian priority, although it should cause him no difficulty, for he clearly states that the six test explosions of the American "castle" series in 1954 were of usable bombs. The important point, of course, is that the overwhelming majority of Americans may have missed such arcane arguments entirely, not needing York's rebuttal to reassure them that "we were first"; how indeed would we have behaved if most Americans had thought it was a "dead heat" or that "Moscow had won"?

GEORGE H. QUESTER

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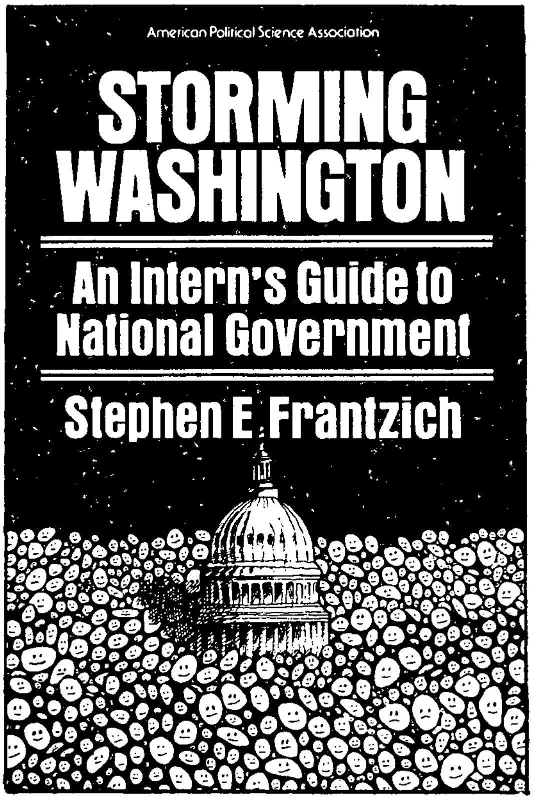

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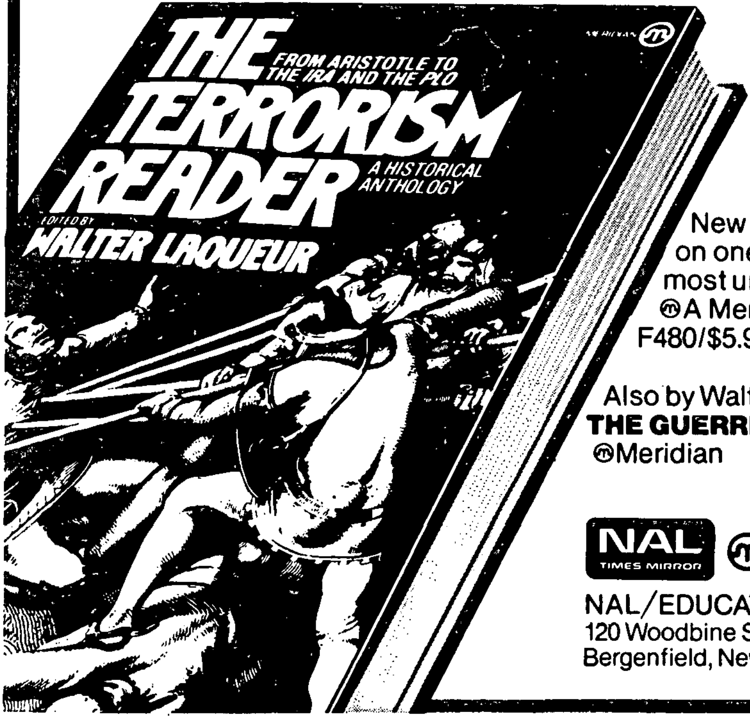

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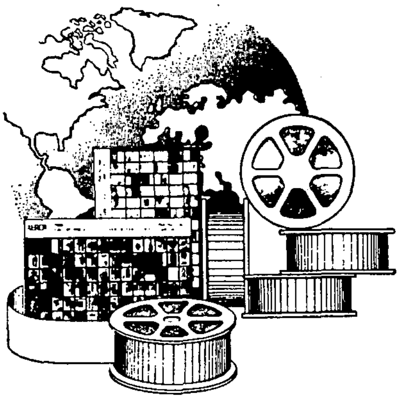

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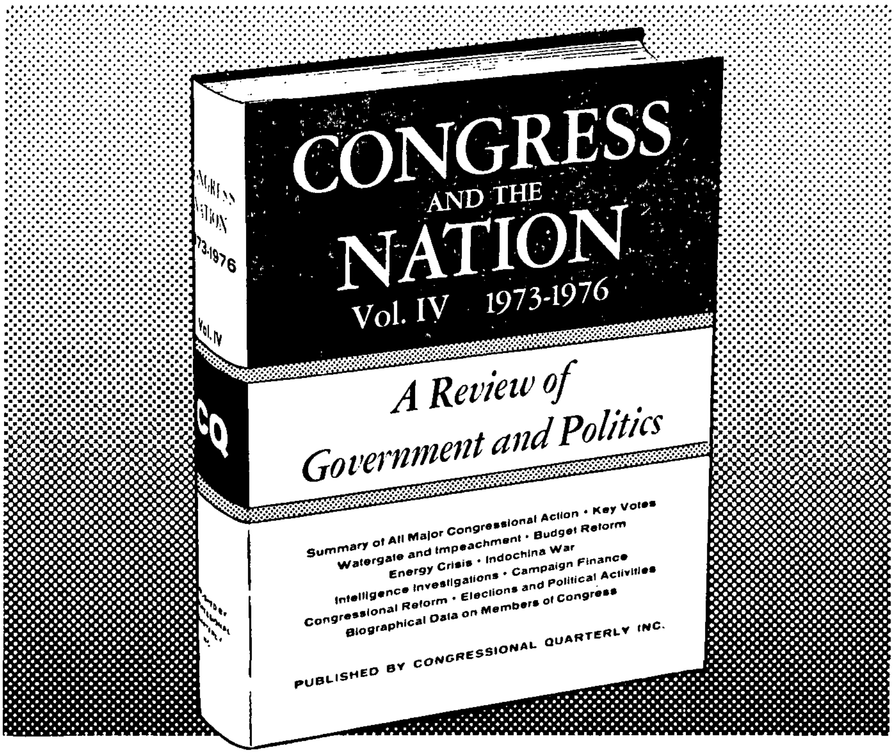
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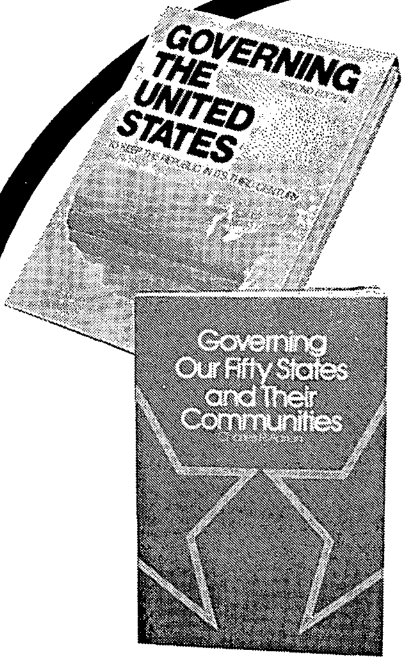
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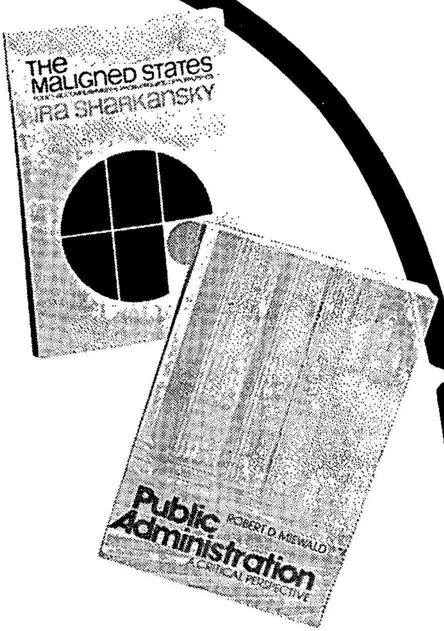

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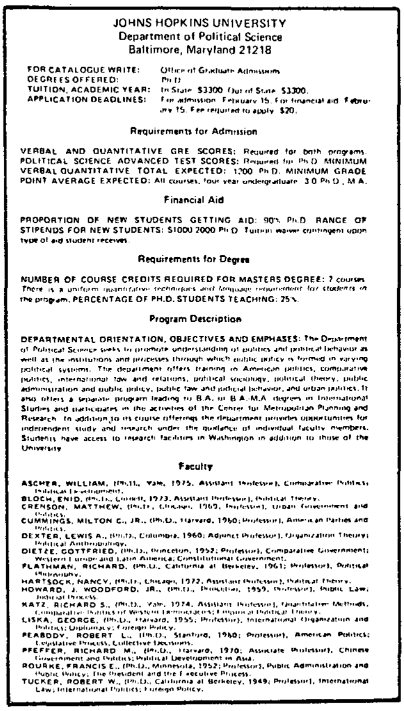

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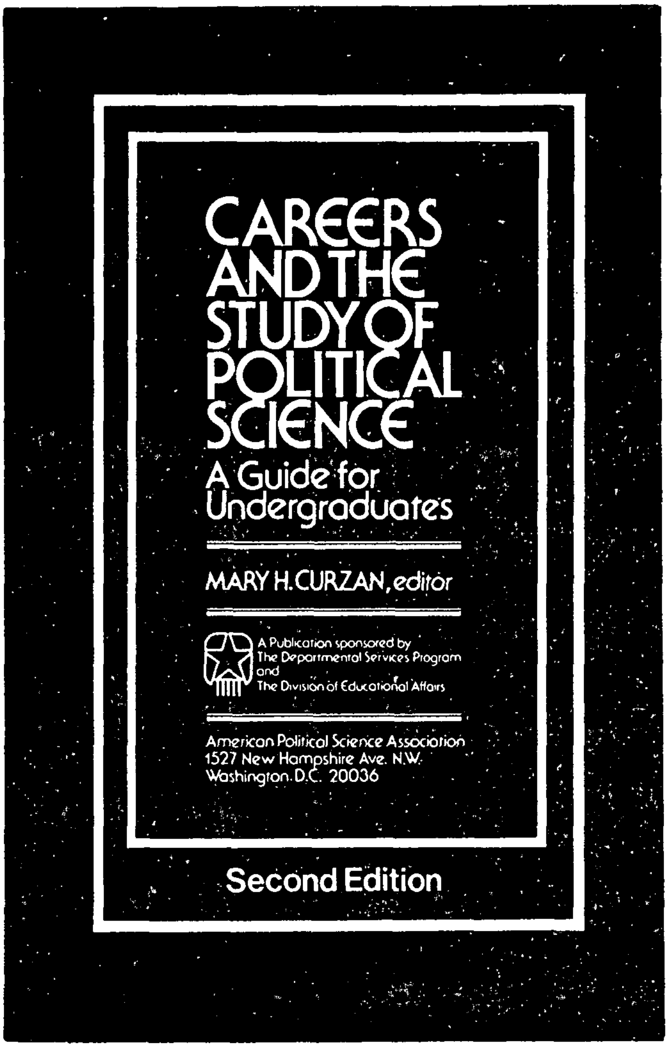

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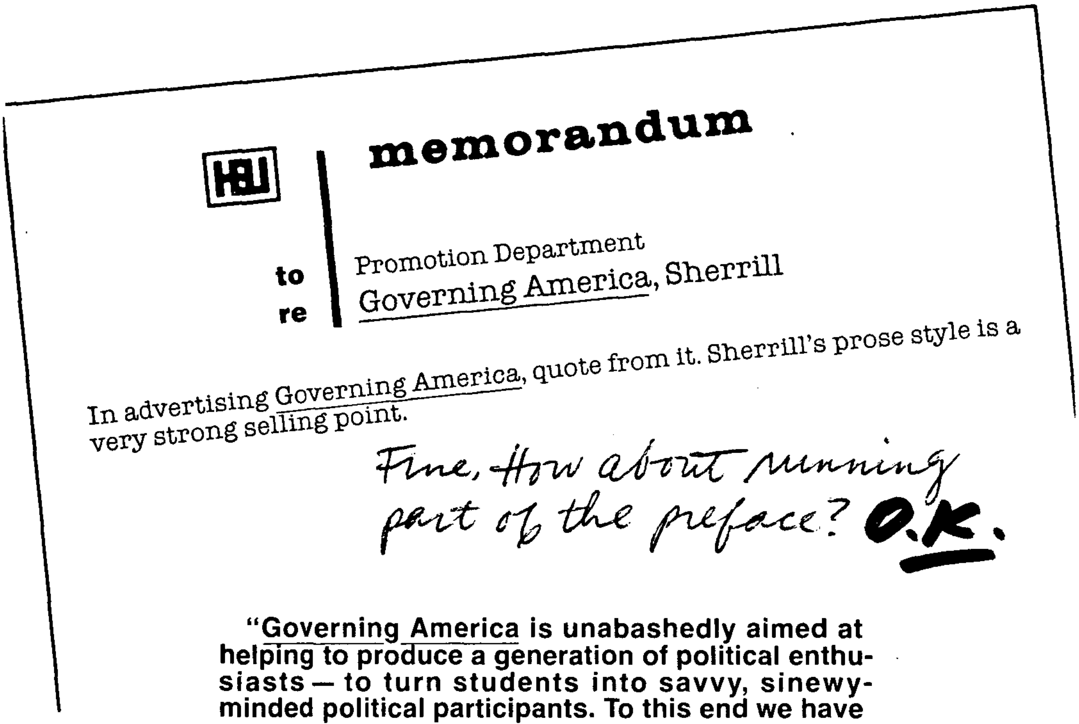

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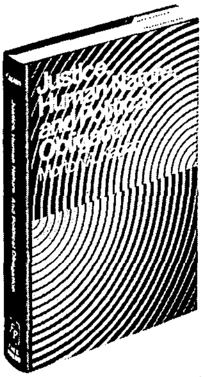

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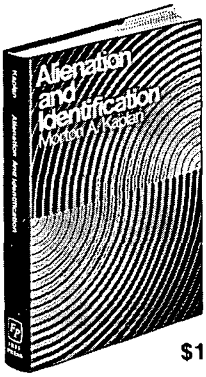

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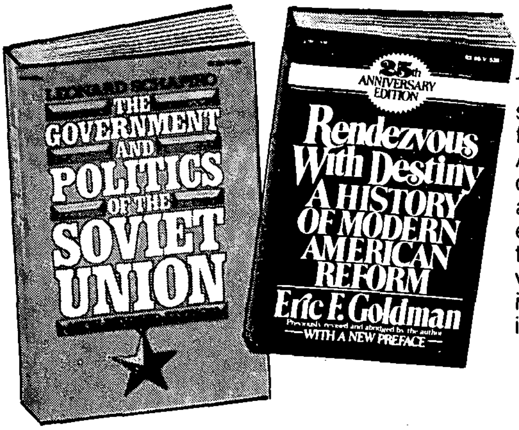
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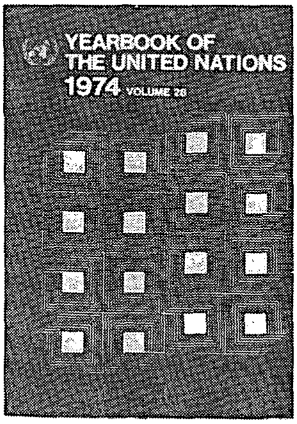

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